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The American Historical Review appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in Ballantine Hall, Indiana University, Bloomington, Indiana 47405.

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes over \$30,000, \$50.00 annually; \$20,000-\$29,999, \$42.00; \$15,000-\$19,999, \$35.00; \$10,000-\$14,999, \$25.00; below \$10,000 and joint memberships \$15.00; associate (non-historian) \$25.00; life \$1,000. The proportion of dues allocated to the *AHR* is \$17.00. Subscription rates effective for volume 85: Class I, *American Historical Review* only, United States, Canada, and Mexico \$39.00, foreign \$43.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—I(a) and 2(a)—immediately preceding the advertisements.

Information concerning the submission of manuscripts and other matters of interest to authors and reviewers is contained on page 2(a), immediately preceding the advertisements.

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The Association cannot accommodate changes of address that are effective only for the summer months.

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“A Place on the Ballot”: Fusion Politics and Antifusion Laws

PETER H. ARGERSINGER

ONLY IN RECENT YEARS have historians seriously investigated the institutional framework of the American electoral system and begun to examine the political effects of ballot forms, voting systems, and suffrage requirements. In particular, some scholars have sought to explain the dramatic changes in political behavior that occurred around the turn of this century as “unintended consequences” of reforms in the structural properties of the electoral system rather than as a reflection of any larger development. While illuminating the political results of such institutional changes, these scholars have largely ignored the political context within which the changes evolved. Thus, they have regarded those structural modifications as essentially apolitical or nonpartisan and have sharply rejected any view that change stemmed from an “antidemocratic conspiracy” to control the political system.¹ Yet, at least one little-known development in the

A preliminary version of this article was presented at the annual meeting of the American Historical Association in Dallas, December 28–30, 1977. I wish to thank the principal commentator at that time, Howard W. Allen, for his helpful criticism. I also wish to express my appreciation to Jo Ann E. Argersinger for advice and assistance.

¹ For present purposes, this “legal-institutionalist” school is best approached in terms of the reaction to Walter Dean Burnham’s “The Changing Shape of the American Political Universe,” *American Political Science Review*, 59 (1965): 7–28. Burnham argued that sharp declines in turnout and increases in split-ticket voting and other indexes of partisan volatility and voter marginality reflected the establishment of corporate political hegemony in the realignment of 1896 and a consequent breakdown in party organization and competition coupled with a rise in voter alienation. For major rejoinders, see Jerrold G. Rusk, “The Effect of the Australian Ballot Reform on Split Ticket Voting, 1876–1908,” *ibid.*, 64 (1970): 1220–38; and Philip E. Converse, “Change in the American Electorate,” in Angus Campbell and Philip E. Converse, eds., *The Human Meaning of Social Change* (New York, 1972), 263–337. For the continuing controversy, see Burnham, *Critical Elections and the Main-springs of American Politics* (New York, 1970); Burnham and Rusk, letters in *American Political Science Review*, 65 (1971): 1149–57; Burnham, “Theory and Voting Research: Some Reflections on Converse’s ‘Change in the American Electorate,’” *ibid.*, 68 (1974): 1002–23; Converse, “Comment on Burnham’s ‘Theory and Voting Research,’” *ibid.*, 1024–27; Rusk, “Comment: The American Electoral Universe: Speculation and Evidence,” *ibid.*, 1028–49; and Burnham, “Rejoinder to ‘Comments’ by Philip Converse and Jerrold Rusk,” *ibid.*, 1050–57. John J. Stucker, like Rusk a former student of Converse at the University of Michigan, has joined Rusk in two further contributions to the legal-institutional theory of electoral change: “The Effect of the Southern System of Election Laws on Voting Participation: A Reply to V. O. Key, Jr.,” in Joel H. Silbey *et al.*, eds., *The History of American Electoral Behavior* (Princeton, 1978), 198–250; and “Legal-Institutional Factors in American Voting,” in Walter Dean Burnham *et al.*, eds., *A Behavioral Guide to the Study of American Electoral History* (Cambridge, Mass., forthcoming). For Burnham’s most recent and developed statement, see his “The System of 1896: An Analysis,” in Paul Kleppner *et al.*, eds., *The Evolution of American Electoral Systems* (Westport, Conn., forthcoming). Finally, for an evaluation of the behavioral and legal-institutional positions in light of the decline in voter turnout in the early twentieth century, see Paul Kleppner and Stephen C. Baker, “The Impact of Registration Requirements on Electoral Turnout, 1900–1916: Multiple Tests of Competing Theories,” paper delivered at

electoral reform of the 1890s involved a conscious effort to shape the political arena by disrupting opposition parties, revising traditional campaign and voting practices, and ensuring Republican hegemony—all under the mild cover of procedural reform. This development was the adoption of so-called antifusion laws, which also altered the political behavior characteristic of the Gilded Age, with varying effects on the role of third parties, modes of political participation, and the electoral process itself.

FUSION, OR THE ELECTORAL SUPPORT OF A SINGLE SET of candidates by two or more parties, constituted a significant feature of late nineteenth-century politics, particularly in the Midwest and West, where full or partial fusion occurred in nearly every election. Such fusions customarily involved a temporary alliance between third parties and the weaker of the two major parties, usually the Democrats in the Midwest and West. In the 1878 congressional elections, however, Indiana Greenbackers fused in one district with Democrats and in another with Republicans. That fusion often seemed to have a life of its own was evident in the Greenback effort of 1884 to arrange fusions in each state with whatever party was in the minority.² Fusion plans were generally undertaken, nevertheless, to promote the needs of the major party and were generally initiated or avoided according to the calculations of its politicians rather than those of the leaders of the evanescent third parties. Thus, the Republicans sometimes arranged fusions in the South but retreated whenever their participation in such a campaign might work against the Democratic divisiveness they sought to exploit. Similarly, in the West, Democrats repeatedly fused *on* third party tickets even over the bitter opposition of independents—that is, any third party followers in the nineteenth-century usage—who feared absorption by the major party or accusations of ideological betrayal. But, if fusion sometimes helped destroy individual third parties, it helped maintain a significant third party tradition by guaranteeing that dissenters' votes could be more than symbolic protest, that

the annual meeting of the American Political Science Association, Washington, D.C., August 1979. Converse has noted that Burnham's "conspiratorial interpretation" prompted his own work; "Comment on Burnham's 'Theory and Voting Research,'" 1024. Much of Rusk's work seems similarly motivated; see his "Comment: The American Electoral Universe," 1045–46. In 1974 Burnham backed away from suggestions of a conspiracy; "Theory and Voting Research," 1022. In one prominent exception to the "nonpartisan" thesis of the legal-institutionalist school, however, J. Morgan Kousser has argued that "the cross-fertilization and coordination" between Democratic movements to quash political opposition legally in the South "amounted to a public conspiracy"; see Kousser, *The Shaping of Southern Politics: Suffrage Restriction and the Establishment of the One-Party South, 1880–1910* (New Haven, 1974), 39. Rusk was prepared to "find a conspiracy which used legal means to control the system" within the Democratic South but strongly denied that one existed among Republicans in the North. He quite rightly recognized that the "paramount" issue in determining the nature of electoral change is that of "legislative intent"—"who urged the passage of these laws and why?" See his "Comment: The American Electoral Universe," 1045–46. The present essay will concentrate on those two questions in explaining one particular Northern electoral development.

² *Appleton's Annual Cyclopaedia, 1878* (New York, 1879), 443; *Chicago Daily Tribune*, September 4, 1884; and Fred E. Haynes, *Third Party Movements since the Civil War* (1916; reprint ed., New York, 1966). An extreme example of the complexity of fusion politics came in North Dakota in 1890 when the Independents fused with the Prohibitionists to nominate candidates for governor and auditor, after which this coalition fused *on* Republican nominees for lieutenant governor and congressman and Democratic nominees for secretary of state and attorney general. *Appleton's Annual Cyclopaedia, 1890* (New York, 1891), 629.

their leaders could gain office, and that their demands might be heard. Most of the election victories normally attributed to the Grangers, Independents, or Greenbackers in the 1870s and 1880s were a result of fusion between those third party groups and Democrats. That some politicians regarded fusion as a mechanism for proportional representation is not surprising.³

Fusion was a particularly appropriate tactic given the period's political culture. Voter turnout was at a historic high, rigid party allegiance was standard, and straight-ticket voting was the norm. Partisanship was intense, rooted not only in shared values but in hatreds engendered by cultural and sectional conflict. Changes in party control resulted less from voter conversion than from differential rates of partisan turnout or from the effect of third parties. Although the Republicans continued to win most elections, moreover, the era of Republican dominance had ended in the older Northwest by 1874 and had been considerably eroded in the states farther west by the 1880s, so that elections were bitterly contested campaigns in which neither major party consistently attracted a majority of the voters.⁴ Minor parties regularly captured a significant share of the popular vote and received at least 20 percent in one or more elections from 1874 to 1892 in more than half of the non-Southern states. Even where their share was smaller, it represented a critically important proportion of that electorate. Between 1878 and 1892 minor parties held the balance of power at least once in every state but Vermont, and from the mid-1880s they held that power in a majority of states in nearly every election, culminating in 1892 when neither major party secured a majority of the electorate in nearly three-quarters of the states.⁵ By offering additional votes in a closely divided electorate, fusion became a continuing objective not only of third party leaders seeking personal advancement or limited, tangible goals but also of Democratic politicians inter-

³ *New York Herald*, March 12, August 13, 1892; *Chicago Daily Tribune*, September 2, 1884; and Haynes, *Third Party Movements*. Also see Lee A. Dew, "Populist Fusion Movements as an Instrument of Political Reform, 1890-1900" (M.A. thesis, Kansas State Teachers College, Pittsburg, 1957). Fusion was not, of course, always successful, but it did offer the best chance of overcoming the Republicans. As one South Dakota Republican observed, "No fusion means Republican victory"; *Brookings County* (S.D.) *Press*, September 29, 1892. Even when defeated the policy of fusion caused a great deal of uncertainty within Republican ranks. In 1884, for instance, Republican Senator William B. Allison of Iowa warned party "managers in the East that this fusion of the Democrats with [Benjamin F.] Butler's forces in the West would require some attention and that we could not afford to rest on our oars with the field combined against us"; *Chicago Daily Tribune*, September 7, 1884.

⁴ For general discussions of the period's political culture, see Burnham, "Changing Shape of the American Political Universe"; Paul Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850-1900* (New York, 1970) and *The Third Electoral System, 1853-1892: Parties, Voters, and Political Cultures* (Chapel Hill, 1979); and Melvyn Hammarberg, *The Indiana Voter: The Historical Dynamics of Party Allegiance during the 1870s* (Chicago, 1977).

⁵ These conclusions as to the political importance of minor parties are derived from data that Paul T. David recorded for gubernatorial and presidential elections in the thirty non-Southern states; see his *Party Strength in the United States, 1872-1970* (Charlottesville, Va., 1972), 102-286. Even these statements underestimate the role of minor parties, because David systematically adjusted his data to discount the minor parties precisely when they engaged in fusion. I have made allowances for this adjustment only in a few obvious instances, as in the 1892 presidential returns for North Dakota or Wyoming. I have focused on state elections here, because electoral laws were a function of individual state legislatures. Paul Kleppner emphasized the same point from a wider perspective when he wrote, "The mean vote cast for minor parties in both the 1876-88 and 1876-92 sequences of biennial elections exceeded the major-party mean partisan lead in the Midatlantic, the East North Central, the West North Central, and the Western regions of the country, as well as in the United States as a whole"; *The Third Electoral System*, 239.

ested in immediate partisan advantage. The tactic of fusion enabled Democrats to secure the votes of independents or disaffected Republicans who never considered voting directly for the Democracy they hated; it permitted such voters to register their discontent effectively without directly supporting a party that represented negative reference groups and rarely offered acceptable policy alternatives.

The use of separate party ballots constituted another feature of the political culture of the Gilded Age that facilitated fusion. Each party printed and distributed its own ballot, without the necessary involvement of either state officials or the candidates themselves. The ballots, or tickets, were strips of paper on which only the names of the candidates of that party appeared. The individual voter could remain ignorant of the nominees of other parties; he merely had to deposit his party ticket in the ballot box, without studying or, in some states, even marking it. This election system allowed partisans of fusing parties to cast their votes without explicitly acknowledging their shared behavior or its significance, and it enabled a party to pursue fusion with an unwilling partner.

Given their vulnerability to fusion politics, Republicans continually sought to prevent cooperation among their opponents. Repeatedly, they pointed out the contradictions in the platforms of the different groups contemplating fusion and urged members of each to adhere to their own principles rather than to fuse with groups holding obviously different aims. Although the Republican motive was transparent, the argument held considerable force, particularly for conservative Democrats, for those Democrats and third party followers who believed in the representative nature of parties and nominations (not uncommon among minority political groups), and for those third party supporters who were interested in the development of their party and realized their reform goals required more than just immediate and perhaps counterproductive electoral victories.⁶ At times Republicans tried to encourage these antifusion elements among their opponents by going beyond such attempts to incite partisan prejudice and actually subsidizing their activities and party newspapers. In 1878 Indiana Republicans even underwrote a separate campaign by the Greenbackers, hoping thereby to draw votes from the Democrats. One final, if perhaps unofficial, tactic to sabotage fusion was demonstrated in Michigan's legislative elections of 1884, when Republicans distributed "bogus tickets calculated to deceive the greenbackers and democrats" by substituting the names of the Republican nominees for the fusion candidates on what otherwise appeared as a regular, fusion-party ballot.⁷

The effectiveness of this type of ballot trickery in disrupting fusion was easily surpassed by the possibilities inherent in the Australian ballot system. The presidential election of 1888, with its widespread incidents of bribery, intimidation,

⁶ For examples of such Republican appeals, see the *Portland Morning Oregonian*, October 25, 26, 27, 28, 1892; and the *Minneapolis Tribune*, October 12, 20, 21, 1892. For an examination of the issue of representativeness in parties, see Austin Ranney, *Curing the Mischiefs of Faction: Party Reform in America* (Berkeley and Los Angeles, 1975).

⁷ *Detroit Evening News*, November 1, 1884. Morton Keller, *Affairs of State: Public Life in Late Nineteenth-Century America* (Cambridge, Mass., 1977), 282; and *Des Moines Farmers Tribune*, August 18, 1897.

and fraudulent voting, provoked a reaction against the partisan excesses possible in the party-ballot system of voting and helped spur most states toward adopting the Australian ballot, long advocated by a number of disparate groups. This system did more than merely ensure secrecy for the voter. It also provided for an official ballot printed at public expense and distributed only by public election officers at the polling place. The system featured a blanket ballot, moreover, which contained the names of all of the candidates legally nominated by any party. The candidates' names were arranged on the ballot in one of two general patterns, the office-bloc or the party-column format. On the office-bloc ballot, candidates were grouped under the name of the office sought and their partisan affiliations were shown. The voter made his choice for each office by marking a square corresponding to the appropriate candidate. On the party-column ballot, candidates were grouped by party and listed in parallel columns. In some states the ballot laws even placed emblems or vignettes at the head of the columns to enable the voter to distinguish more easily the separate parties. Finally, lawmakers frequently added to the ballot a device to facilitate straight-ticket voting, a party circle, which, when marked, constituted a vote for the entire party ticket. These developments represented legislative efforts to retain some of the familiar, partisan features of the old ballot system while providing the secret and official characteristics of the new.⁸

By providing for public rather than partisan control over the ballots and by featuring a blanket ballot, the Australian system opened to Republicans, given their dominance in state governments, the opportunity to use the power of the state to eliminate fusion politics and thereby alter political behavior.⁹ The Republicans' modifications of the Australian ballot were designed to take advantage of the attitudes and prejudices of their opponents and were based on a simple prohibition against listing a candidate's name more than once on the official ballot. This stipulation, Republicans believed, would either split the potential fusion vote by causing each party to nominate separate candidates or undermine the efficacy of any fusion that did occur, for in this time of intense partisanship many Democrats would refuse to vote for a fusion candidate design-

⁸ The scholarly literature on the development of the Australian ballot is surprisingly thin and analytically unsophisticated. But see L. E. Fredman, *The Australian Ballot: The Story of an American Reform* (East Lansing, Mich., 1968); and Eldon C. Evans, *A History of the Australian Ballot System in the United States* (Chicago, 1917).

⁹ It is not asserted here that Republicans enacted the Australian ballot in the first place for such partisan purposes, and Rusk's attempt to deny the partisan effect of the Australian ballot by noting that both Democrats and Republicans voted for the initial reform in state legislatures is unsatisfactory. Rusk, "Comment: The American Electoral Universe," 1045. The law itself and its basic provisions for a secret, public ballot did not become the object of contention (except in rare cases as in New York) so much as the modifications of the Australian ballot system and the use that could be made of them did. As one opponent of subsequent Republican ballot changes in South Dakota said, "The real trouble is the change from the law as it originally stood." Another Dakota correspondent noted that each legislature after the one that had enacted the Australian ballot "has been tinkering at the law, and . . . wrapped the ballot in technicalities." After a Populist governor urged "that the old safe-guards which have been one-by-one repealed since the passage of the original law be reinstated," a Populist legislature adopted a law providing "for a return to the method when the Australian system was first adopted." Sioux Falls (S.D.) *Argus-Leader*, January 11, 1895; *Chicago Daily Tribune*, January 4, 1897; *South Dakota Senate Journal* (Pierre, 1897), 43-44; and Yankton (S.D.) *Press & Dakotan*, February 11, 1897. For opposition in New York to the Australian ballot itself on practical, ideological, and partisan grounds, see the discussion in Herbert J. Bass, "I Am a Democrat": *The Political Career of David Bennett Hill* (Syracuse, 1961), 96-101, 128-30, 133-35, 147-48, 151-53.

nated "Populist" and many Populists would feel equally reluctant to vote for a "Democrat."¹⁰ Related regulations could restrict straight-ticket voting by fusionists or even eliminate one of the fusing parties, antagonizing its partisans and causing them either to oppose the fusion arrangements or to drop out of the electorate altogether. Given the closely balanced elections of the late nineteenth century, the elimination of even a small faction of their political opponents because of ideology, partisanship, or social prejudice would help guarantee Republican ascendancy. Although other ballot adjustments increased its effectiveness, this simple prohibition against double listing became the basic feature of what the Nebraska supreme court described as a Republican effort to use the Australian ballot as a "scheme to put the voters in a straight jacket."¹¹

Publicly, Republicans defended this prohibition as necessary for achieving equal treatment, efficiency, and an end to political corruption, and they insisted that technically it did not interfere with nominations or voting. But, given what one Wisconsin judge called "the strength of party ties" and the reality that "political rights are universally exercised through party organizations," the logic of the law lay in his conclusion that "its only purpose is to prevent fusion." The law, he continued, "will prevent no illegal vote from being cast, nor will it stop any corrupt practice, nor in any way preserve the purity of the ballot." It was designed, instead, to interfere with "the freedom of action of the party . . . [and] of the citizens who compose that party." The Republican judicial rejoinder, of course, was that "mere party fealty and party sentiment, which influences men to desire to be known as members of a particular [political] organization, are not the subjects of constitutional care."¹² The law, then, was intended to promote the dissolution of party ties while giving Republicans the residual benefits of them.

THE POSSIBILITIES OF ADJUSTING THE BALLOT SYSTEM in this direction became evident during the 1892 presidential campaign, the first held under the original Australian ballot system. That campaign marked as well the initial national appearance of the most important third party of the late nineteenth century, the People's Party, which had its greatest appeal to economically distressed farmers in the Western states, traditionally controlled by Republicans. In an effort to increase the electoral chances of its presidential candidate, Grover Cleveland, the Democratic National Committee urged party officials in several Western states to withdraw their nominees for the electoral college and fuse on the Populist nominees, thereby denying Republicans the electoral votes that Cleveland would be unable to capture for himself.

¹⁰ It was "well known," one newspaper observed, that many voters would not vote for a candidate unless he were listed on their ticket. "This may be a prejudice, but it is not an unworthy one in a community where party government is recognized." *Detroit Free Press*, March 15, 1895. An Ohio Greenbacker had made the same point earlier and more graphically: "Men would as soon cut off their right hands almost as vote a Democratic ticket." *Cincinnati Enquirer*, August 22, 1877, as quoted in R. C. McGrane, "Ohio and the Greenback Movement," *Mississippi Valley Historical Review*, 11 (1925): 535.

¹¹ *State v. Stein* (Neb.), 53 N.W. Rep. 999.

¹² *State v. Anderson* (Wisc.), 76 N.W. Rep. 482.

The responses in Oregon and Minnesota proved most significant. Both states were controlled by Republicans although the GOP represented only a minority of voters in each. Hoping to arrange a successful fusion, Democratic officials withdrew one of their four nominees in Oregon and four of their nine nominees in Minnesota, replacing them with candidates nominated by the Populists. In both states many Populists denounced the Democratic maneuver, worrying that, as one Minnesota Populist elector said, the tactic "will hurt the People's Party rather than help it, as a great many in that party were formerly Republicans, and . . . will have a tendency to drive them back to the old party." Many Democrats also complained of the arrangement, but gradually most concluded that, although fusion with the Populists was distasteful, "in the present case, the end justifies the means," as the Oregon state chairman observed.¹³ The initial Republican reaction to these fusion arrangements also followed the customary pattern. To their own partisans Republican leaders stressed two contradictory conclusions: fusion was a confession of Democratic weakness, but Republicans would have to turn out in greater numbers to vote it down. To Democratic and Populist voters, the Republican leaders appealed separately, insisting that fusion required their party to subordinate its own sacred principles and able candidates to those of the other party.¹⁴

These fusion campaigns differed from previous ones, however, because of the Republicans' partisan implementation of the Australian ballot law, which both states had enacted in 1891. In adopting the office-bloc form of ballot, the Oregon legislature had also prohibited the name of any candidate from appearing more than once on the ballot. Perhaps this provision had seemed a logical corollary to the ballot type, for it excited no comment at the time. When in 1892 Democratic officials recognized the implications of that clause for their fusion plans, they argued that another provision, which permitted the names of electoral college candidates to be grouped by parties, allowed the fusionist elector, Nathan Pierce, to be listed with Democrats as well as with Populists on the ballot. Republicans countered that Pierce's name could be listed only once and identified as a "Populist" or at most "Populist-Democrat," expecting that the word "Democrat" would be a signal to Republican-Populists to scratch the name and that the Populist designation would alienate some Democratic voters: "a very pretty jungle," in the words of one Republican editor.¹⁵

The question of ballot form appeared so late in the campaign that there was no time to secure a legal decision, and county clerks turned to party leaders for guidance in printing the official ballots. The ballot devised by the Democratic state committee and subsequently copied by Democratic county clerks listed Pierce's name in both the Democratic and Populist groupings, while those county clerks who followed the instructions of the Republican state chairman

¹³ *Minneapolis Tribune*, October 13, 19, 24, 1892; and *Portland Morning Oregonian*, October 28, November 2, 1892.

¹⁴ *Portland Morning Oregonian*, October 25, 26, 27, 28, November 2, 1892; and *Minneapolis Tribune*, October 12, 20, 21, 1892.

¹⁵ *Portland Morning Oregonian*, October 27, 28, 30, 1892.

listed Pierce only among the Populist nominees, though designating him with both party affiliations.¹⁶

Because of this singular ballot situation, Oregon's election results revealed both the value of fusion and the effect of ballot format in shaping electoral outcomes and disrupting fusion coalitions. The Republicans won three of the four electoral votes, averaging 35,000 votes for their candidates. The straight Populist candidates averaged 27,000 and the straight Democrats 14,000. Had Pierce received the full vote of both parties he would have been an easy victor with approximately 41,000 votes, but he squeaked through with only 35,811. Regression analysis indicates that in those counties in which his name was listed under both Democratic and Populist groupings virtually all Populists voted for their fellow partisan, while 92 percent of the Democrats also supported Pierce, an indication of some hostility to fusion but also of a general willingness to vote the Democratic ticket and all who were designated on it. But, in those counties in which Pierce's name was listed on the ballot only once (under the Populist group), 9 percent of the Populist voters refused to support a Populist who was also labeled a Democrat, although he was identified as a Populist and listed with the other Populist electors. And 29 percent of the Democrats refused to vote for a Democratic candidate who was also listed as a Populist.¹⁷ Republican expectations as to voter behavior had proved accurate.

While Oregon provides the most revealing evidence of the effect of ballot format on voting behavior and fusion politics, events in Minnesota proved more immediately influential for electoral reform. Like that in Oregon, the ballot law in Minnesota also established the office-bloc format, but without the restriction on listing candidates' names more than once. In preparing the official ballot for 1892, however, the Republican secretary of state simply proceeded as though that were a legal requirement, grouping the five straight Democratic electors separately and scattering the four endorsed Populists among the five other Populists, though designating them as both Populist and Democratic. Democratic officials charged that the Republican ballot design was constructed to "render it more difficult for the voter to cast his vote according to his preference" and sought a court order to compel the double listing of fusion electors. Democratic lawyers argued that, as drawn up, the official ballot would disfranchise twenty thousand voters. But, since the ballots were already printed, the court was confronted with a Republican *fait accompli*, the reversal of which would have required a postponement of the election itself, and accordingly the court judiciously ruled that it had no jurisdiction in the matter.¹⁸

Ignoring their own structural revolution, Republicans crowed that "the court

¹⁶ *Ibid.*, October 28, 30, 1892.

¹⁷ *Appleton's Annual Cyclopaedia*, 1892 (New York, 1893), 615; and *Portland Morning Oregonian*, November 11, 1892, January 6, 1893. Estimates of voter behavior were derived from ecological regressions calculated for those twenty-seven (of thirty-two total) counties for which firm evidence exists as to the ballot format employed. For the best introduction to this technique, see J. Morgan Kousser, "Ecological Regression and the Analysis of Past Politics," *Journal of Interdisciplinary History*, 4 (1973): 237-62; and W. P. Shively, "Ecological Inference: The Use of Aggregate Data to Study Individuals," *American Political Science Review*, 63 (1969): 1183-96.

¹⁸ *Minneapolis Tribune*, October 16, 18, 19, 1892.

and secretary of state do not propose to become the cat's paws of the fusion schemers and turn the ballot upside down to suit their political ends." Democrats found consolation in the publicity their court case had engendered: it called voter attention to the structure of the ballot and indicated how Democrats would have to vote. Indeed, even Republicans argued that repeated instruction in "the science and art of casting a ballot under the Australian system" would be more valuable than "profound dissertations on the tariff and the currency." The election results validated that estimate of the importance of the ballot. The straight Democratic electors averaged 101,000 votes and the straight Populists 29,000; their combined total would have easily defeated the Republicans' 113,000. Yet the four fusion electors received only 110,000 votes, the drop of 20,000 that Democratic officials had predicted, which allowed the minority Republicans to sweep to complete victory.¹⁹

The massive vote differentials in these states were largely a function of institutional change in the voting system, but they also involved a behavioral component, for the ballot arrangements were advertised and explained extensively, and voters could have selected the fusion candidates if they had been willing to vote with a different party. That some voters were obviously unwilling to ally themselves even symbolically with another party testifies again to the nature and strength of partisan affiliation in the political culture of the time. In evaluating the decline in the fusionists' votes, one Minnesota election judge observed, "It matters not whether this was the result of sharp practice or not, the fact remains . . . they were cheated out of their votes" by the "system of voting."²⁰ But, significantly, the decline was not an "unintended consequence" of ballot change but rather resulted from "sharp practice." The institutional change had been purposely designed to exploit the observed behavioral patterns in the political culture and did not represent some abstract or disinterested impulse toward "reform."

This basic reality became increasingly obvious from the reactions in other states to the Minnesota experience. Neighboring Wisconsin, traditionally Republican, had gone Democratic in 1892 because of local circumstances. Fusion had occurred at several levels and, as one Republican editor complained, "the labor party, or people's party, or Farmers' Alliance, assisted to place in power" the Democrats, and "without those voters the democratic party is in a minority in the state." To protect these voters and their own new position, Democratic legislators amended the election law in 1893 specifically to provide for dual ballot listings in the event of fusion nominations.²¹

By 1893, Michigan had perhaps experienced more consistent fusion politics than any other state, and the new, Republican legislature decided to revise the Australian ballot law that had been enacted by its Democratic predecessor. Although there was considerable discussion about the need "to purify elections

¹⁹ *Ibid.*, October 10, 19, 1892. John D. Hicks, "The People's Party in Minnesota," *Minnesota History Bulletin*, 5 (1924): 545.

²⁰ *Minneapolis Tribune*, November 17, October 19, 1892.

²¹ *Madison Wisconsin State Journal*, January 3, 1893; and *The Registry and Election Laws of the State of Wisconsin* (Madison, Wisc., 1894), 26.

and prevent fraud thereat," the GOP's objective clearly was, as one Democrat observed, to "purify elections according to the Republican idea of purity, and prevent frauds by all other parties." One Republican legislator, at least, was candid: "We don't propose to allow the Democrats to make allies of the Populists, Prohibitionists, or any other party, and get up combination tickets against us. We can whip them single-handed, but don't intend to fight all creation."²² The Republicans' solution was ballot manipulation, a tactic they first applied retroactively by unseating non-GOP legislators whose names had been listed upon more than one ticket. The presiding officer refused even to entertain Democratic protests against this "revolutionary action," but the state supreme court partially restrained the Republican majority by upholding the legality of ballots with dual listings. The Republicans then countered by moving to amend the state's election law by prohibiting double listing of candidates' names on the ballot. This effort also failed, but by only three votes. All forty-eight votes in favor of the bill were Republican; all Democrats and Populists voting opposed the measure.²³

This Republican attempt to unseat legislators reveals an important aspect of the movement for the legal disruption of fusion politics. Fusion occurred most often in local and state-legislative contests, where the candidates' personal popularity was more likely to displace partisan issues in determining voters' preferences. In Michigan, for example, fusion did not materialize in the presidential race of 1892, and the Populists made a negligible showing. But Populist strength was regional and often proved decisive in local contests. In the legislature of 1893, which first debated the issue, twenty-one of the thirty-one Democratic and Populist state representatives had been elected through fusion, while twenty-six Republican representatives had been elected over fusion opponents. At least twenty more Republicans were elected only by plurality votes and would have been defeated if their opponents had successfully fused. Thus, a major object of antifusion legislation was at times local, not national, politics. When Michigan did, in fact, later enact an antifusion law, its passage owed some of its immediate support to a pending special congressional election in southwestern Michigan, where Populists were strongest and where the Republican candidate, the presiding officer of the state senate, was opposed by the fusion nominee of Populists, Prohibitionists, Silverites, and Democrats. This local nature of fusion was what prompted interest in antifusion legislation in those states where, at the aggregate level, it did not seem necessary or important. But a focus on the small total number of Populists in large industrial states like Michigan or Wisconsin is misleading in other ways as well. While Michigan Populists polled only 4.3 percent of the total state vote in 1892, that proportion gave them the balance of power in the closely contested electorate; when delivered to Democratic can-

²² *Detroit Free Press*, February 1, January 5, 1893.

²³ *Ibid.*, January 4, 19, February 15, 16, 25, 1893; *Journal of the House of the State of Michigan, 1893* (Lansing, 1893), 697, 1031; and *Official Directory and Legislative Manual of the State of Michigan, 1893-4* (Lansing, 1893), 706-11.

didates through fusion, as in the contest for attorney general, it sufficed to bring about the only Republican losses on the state ticket.²⁴

South Dakota succeeded where Michigan failed in 1893 in passing the first explicitly antifusion law.²⁵ Constituting only a minority of the voters, the state's Republicans had apprehensively watched developments in Minnesota and devoted their own 1892 campaign "almost exclusively to the business of preventing a fusion" between Populists and Democrats. In 1893 they carried this objective into the legislature and enacted a number of changes in election laws. The most important simply provided that "the name of no candidate shall appear more than once on the ballot for the same office." Related changes similarly designed to frustrate fusion included a prohibition against the withdrawal of candidates shortly before elections, called "the Minnesota plan" of fusion; a provision to treat fusing parties as a single party when appointing election judges; and the replacement of the office-bloc with the party-column format, containing a party circle provision for straight-ticket voting.²⁶ This last modification made more effective the prohibition against double listing, for by requiring party columns a candidate could be identified with only one party affiliation, unlike candidates on the Oregon and Minnesota ballots, and the second party to nominate a candidate would appear on the ballot as having no nominee for that office at all. Those wishing to fuse would thus lose the symbolic protection of voting for their own party and be required to vote as members of another party. When fusion did not involve whole tickets, which was the usual case, fusion voters would also lose the advantage of the party circle and have to check each individual name—a provision that was certain to complicate voting and lead to the invalidation of ballots through improper marking.²⁷

The effects of these new ballot provisions were felt in the state's elections in 1893 and 1894, when, as Republicans observed, they served as a "stumbling block" to their opponents. Populists and Democrats named separate state tickets in order to maintain their parties' organization and independence, though each party conceded that such separation would lead to a Republican victory. The weaker Democrats, in particular, feared that under the new law cooperation with Populists would be "not fusion but absorption." Although fusionist leaders made some local efforts at fusion, they predicted that at least 20 percent of the

²⁴ Richard Harvey Barton, "The Agrarian Revolt in Michigan, 1865-1900" (Ph.D. dissertation, Michigan State University, 1958), 125-51; *Kalamazoo Weekly Telegraph*, October 19, 1892, March 20, 1895; *Official Directory and Legislative Manual of Michigan, 1893-4*, 593-625; and *Appleton's Annual Cyclopaedia, 1892*, 467.

²⁵ The Oregon law of 1891 cannot be so considered for it was passed without apparent recognition of its significance, was still open to contrasting interpretations, and preceded a formal antifusion law enacted in 1895. Both Kentucky and Indiana had early laws that contained provisions resembling those characteristic of antifusion laws but that were really designed to deal with the possibility of nonpartisan nominations by petition rather than with party action. In practice, moreover, their election laws were not interpreted in a fashion to prevent fusion. For a discussion of the local political context surrounding the development of antifusion legislation in South Dakota, consult my "'Confusion to Democracy': Ballot Laws and Politics, 1890-1902," paper delivered at the thirteenth annual Northern Great Plains History Conference, Fargo, N.D., October 27, 1978, pp. 2-6.

²⁶ *New York Times*, October 21, 1892; *South Dakota House Journal* (Pierre, 1893), 862; *South Dakota Senate Journal* (Pierre, 1893), 58, 283-86, 1006; and *Yankton (S.D.) Press & Dakotan*, March 9, 23, 1893.

²⁷ See Sioux Falls (S.D.) *Argus-Leader*, November 4, 1893; *Yankton (S.D.) Press & Dakotan*, November 24, December 8, 1892; and *DeSmet Independent*, as quoted in *Brookings County (S.D.) Press*, November 2, 1893.

Democrats would refuse to vote outside their party name, a fall-off that spelled defeat in a close election. After the expected Republican victory, one Democratic party official observed, "Under the present system of voting as arranged by the Republican party, fusion results in confusion to Democracy."²⁸

Nationally, the Republican success in 1894 led to the passage of antifusion laws by other states in 1895. Oregon Republicans, who had captured a majority in the legislature with only a minority of the popular vote, formally enacted an antifusion statute.²⁹ In neighboring Washington, after successfully campaigning against "fusion schemes," the Republicans applied the force of the one-listing provision in the party-column format to the office-bloc ballot by stipulating that only one party affiliation could be designated for any candidate.³⁰ Michigan Republicans, now in complete control of the legislature, reintroduced their antifusion bill of the previous session and pushed it into law. Although some judges described it as "unconstitutional" and "revolutionary," the state supreme court upheld the measure in the same partisan spirit in which it had been enacted—four Republican judges in the affirmative, one Democrat in dissent.³¹ The Ohio legislature, meeting in 1896, concluded this first legislative flurry with the so-called Dana law, an elaborate measure based upon the customary antifusion ballot requirement. In Ohio, the local focus of antifusion legislation seemed particularly evident, at least initially. In the recent Cincinnati mayoral election, the Republican machine of "Boss" George Cox and Joseph B. Foraker had been challenged by a fusion coalition of Populists, Socialists, laborites, and dissident Republicans that had nearly received the Democratic endorsement as well. The regular Republicans had reacted "as if civilization were at stake." Some legislative observers regarded the subsequent Dana bill, prepared by a Foraker Republican, as primarily designed to prevent just such unified popular revolts against machine rule in municipal elections. Indeed, the Republican legislative majority, so large as to be "dangerous," according to one editor, voted down a proposed amendment to exclude municipal elections from the antifusion provisions.³²

THE LARGER POLITICAL IMPORTANCE of these new antifusion laws was promptly demonstrated in the presidential election of 1896, the pre-eminent fusion cam-

²⁸ *Brookings County (S.D.) Press*, October 19, 1893; and *Sioux Falls (S.D.) Argus-Leader*, August 3, 29, September 5, 6, November 13, 1894.

²⁹ *Appleton's Annual Cyclopaedia, 1894* (New York, 1895), 636; *Appleton's Annual Cyclopaedia, 1895* (New York, 1896), 632; *Oregon House Journal* (Salem, Oreg., 1895), 1007–08; and *Oregon Senate Journal* (Salem, Oreg., 1895), 631, 640.

³⁰ *Spokane Spokesman-Review*, November 1, 10, 1894; *House Journal of the State of Washington, 1895* (Olympia, Wash., 1895), 667–72; and *Senate Journal of the State of Washington, 1895* (Olympia, Wash., 1895), 709. Republican legislators backed this "reform" by a vote of 68 to 1, while Populists opposed it by a margin of 17 to 3.

³¹ *Journal of the Senate of the State of Michigan, 1895* (Lansing, 1895), 112, 373–74, 457, 775–78; *Journal of the House of the State of Michigan, 1895* (Lansing, 1895), 961; *Kalamazoo Weekly Telegraph*, March 20, 1895; *Detroit Free Press*, March 26, 1895; and *Todd v. Election Commissioners*, 104 Mich. 474, 486 (1895).

³² Zane L. Miller, *Boss Cox's Cincinnati: Urban Politics in the Progressive Era* (New York, 1968), 89; *Cincinnati Enquirer*, November 7, 1895; *Cleveland Plain-Dealer*, April 9, 1896; *The Journal of the Senate of the State of Ohio, 1896* (Norwalk, Ohio, 1896), 399–400; and *The Journal of the House of the State of Ohio, 1896* (Norwalk, Ohio, 1896), 689.

paign of the late nineteenth century, when Democrats, Populists, and Silver Republicans fused on the candidacy of William Jennings Bryan. In those antifusion states, like Ohio, in which Democrats constituted by far the major portion of Bryan's supporters, the Populists were sacrificed in a way that not even middle-of-the-road Southerners anticipated when they opposed the Populist endorsement of Bryan.³³ Ohio state election officials announced that the Dana law would eliminate a Populist national ticket from the ballot and, through its party-column and marking procedures, might invalidate ballots that tried to combine support for Bryan with a state or local Populist ticket. To avoid that outcome, Democratic and Populist leaders reluctantly also agreed to fuse on complete state and local tickets. The Democratic state committee withdrew the Democratic nominees for several offices and substituted Populist nominees in exchange for Populist acceptance of the remaining Democratic candidates. All candidates were listed on the ballot only under the Democratic heading. Many Populists, however, were loath to become even nominal Democrats and objected to these arrangements in which their party "was left without a place on the ballot." Defiance County Populists even advocated rescinding Bryan's nomination in order to protect their own party, while other Populists refused to withdraw their nominations. Ultimately, in addition to the designated Democratic ticket, composed of both Democrats and Populists, the official ballot *did* list a severely truncated Populist ticket after all, so that a Populist could not easily vote a straight ticket.³⁴

A second antifusion provision of the Dana law threatened even the Democrats' ability to cast a straight ticket by prohibiting the entry of nominees on one party's ticket when they were certified as members of another party. Accordingly, the secretary of state prepared to split the arduously constructed composite ticket into separate columns after all. Thus, Populist nominees had to declare themselves as Democrats, causing still more disaffection, for the Populists announced that they had "already gone much further than they had wished in order to effect the fusion agreement" and were reluctant "to declare that they are Democrats and thereby destroy absolutely the individuality of the party organization they have been striving to represent." Finally, the Populist state chairman ignominiously had to assert that the Populists had not really made any nominations and to withdraw their certificates; election officials then placed the fusion slate on the ballot as the Democratic ticket.³⁵ Though the fusionists' troubled campaign ended in defeat, the straight Populists fared even worse with the election law. Their officially disavowed ticket failed to attract enough votes

³³ The following discussion involves only those developments that stemmed from antifusion legislation and does not cover any of the quite different difficulties with respect to fusion that Robert Durden has already described well; see his *The Climax of Populism: The Election of 1896* (Lexington, Ky., 1966). The phrase "middle-road," or "middle-of-the-road," referred to those Populists who opposed fusion or cooperation with either major party, which they regarded as being in the gutters of the political system—on each side of those who kept clean and pure in the middle of the road.

³⁴ *Bowling Green (Ohio) Daily Sentinel*, July 17, 28, August 28, September 16, 23, 1896; *Cincinnati Commercial Tribune*, August 8, 12, October 21, 1896; and *Columbus Ohio State Journal*, October 20, 24, 1896.

³⁵ *Cincinnati Commercial Tribune*, October 3, 4, 6, 7, 8, 1896.

to entitle the party to be on the Australian ballot in the future, except by petition. As one Populist disconsolately wrote his national chairman, "We have now in Ohio no People's party, but simply scattered organizations here and there."³⁶

In antifusion states where Populists constituted the majority of Bryan's supporters, roles were reversed but the same difficulties developed. In Oregon, Democrats had to withdraw their electors and accept the Populist ticket as their own. The Populist state chairman explained the result: "Under our statute they surrendered their legal autonomy by this act, so that we have but two parties in this state." The further consequence was that, whereas the Populists and Democrats running separate tickets had together captured 53 percent of the votes in the June state election on much the same issues, Democratic fall-off permitted them to attract only 48 percent of the vote in November under this rankling arrangement.³⁷ In Washington as well, after a committee of lawyers examined the "cunningly devised" election law, the Democrats felt forced to accept the Populist name for the fusion ticket. The outcry against sacrificing the party name was so great, however, that, to the end of the negotiations, it seemed likely that the

³⁶ Hugo Preyer to Marion Butler, March 19, 1897, Marion Butler Papers, Southern Historical Collection, University of North Carolina Library; and *Bowling Green (Ohio) Daily Sentinel*, November 10, 1896. This dissolution of Ohio's Populist party demonstrates the destructive effect on smaller parties of the interaction between the antifusion law and another standard provision of the Australian ballot system. The adoption of the Australian ballot meant of course that disgruntled citizens could no longer simply organize themselves spontaneously and enter the political arena independently by issuing their own party ticket. The use of an official, blanket ballot required the state to establish procedures to regulate the appearance of parties and their candidates on the ballot. This regulation usually involved, *inter alia*, defining a party that could appear on the ballot in terms of its percentage of the total vote in the preceding election. Manipulation of the minimum required percentage often reduced the number of minor parties by directly limiting their ability to present themselves for voter consideration. Petitioning provided an alternative method for gaining party access to the ballot. But, again, some states required unreasonably large numbers of signatures—or even specified a particular geographical distribution of the petitioners—so that, in practice, the candidates would be confined to the larger parties. Once parties were stricken from the ballot (through their candidates) by the operation of the antifusion law, they legally ceased to exist until their partisans successfully petitioned to secure ballot consideration again. Even the Democratic Party had no standing in those states where it fell victim to antifusion regulations; see pages 300–01, below. But small and poor or loosely organized parties faced particular difficulty in regaining an opportunity to appear on the ballot. Members of all political parties were extremely sensitive to this possible consequence of the interaction of the antifusion and other ballot provisions of the electoral law. Unfortunately, not even some of the Populists themselves could resist this legal opportunity to obstruct possible opponents by keeping them off the ballot in the first place. In Kansas, for instance, the regular Populists, after being troubled by a radical (middle-of-the-road) Populist separate ticket in 1896, amended the state's Australian ballot law in the 1897 legislature to quintuple the number of signatures required to gain a ballot position through petition and thereby keep "small bodies of reformers out of politics." Only rarely, of course, were Populists in a position to manipulate the legal parameters of politics, a point perhaps underlined by this same legislature's simultaneous ability to defeat an antifusion bill—on a strict party vote, all Republicans in favor, all Populists and Democrats opposed. *Dubuque (Iowa) Herald*, February 17, 1897; and *Senate Journal: Proceedings of the Senate of the State of Kansas* (Topeka, 1897), 787, 884–85, 1111, 1201. For examples of the more typical major party effort to obstruct new parties through the requirement of an extraordinary number of petition signatures, see Erik Falk Petersen, "The Struggle for the Australian Ballot in California," *California Historical Quarterly*, 51 (1972): 239; and Charles Chauncey Binney, "Merits and Defects of the Pennsylvania Ballot Law of 1891," *Annals of the American Academy of Political and Social Science*, 2 (1892): 751–71, esp. 757n, 758n. Beyond mandating procedures for securing a position on the ballot, the "infamous" Missouri election law required a party to receive one-third of the total votes cast or be "disbarred from all privileges and representation" in the appointment of election judges and clerks. *Kalamazoo Weekly Telegraph*, April 15, 1891. For a striking example of the local exclusion of small parties through legal regulations, see *Bowling Green (Ohio) Daily Sentinel*, March 27, 1896.

³⁷ John C. Young to Marion Butler, March 22, 1897, Butler Papers; and *Appleton's Annual Cyclopaedia*, 1896 (New York, 1897), 628.

Democrats would repudiate the plan, thereby defeating Bryan in a state where victory would have been easy under previous electoral rules.³⁸

Similarly in South Dakota, Democrats recognized that under the ballot law they had to sacrifice their party's organization to secure the state's electoral votes for their party's nominee. Accordingly, they canceled their state convention and adopted the ticket and the name of the People's Party. At the county level, however, Democratic opposition to voting under the Populist name frequently led to a compromise name of "Free Silver" for the local fusion ticket. But this ran afoul of the party-column and party-circle provisions of the ballot law and made it difficult for fusionists to cast a straight ticket for both state and local offices. The likely result under the ballot law, one newspaper predicted dryly, was "the loss of numerous votes for one ticket or the other or . . . loss of both tickets."³⁹

Michigan, with its fusion tradition and moderately strong third parties, furnished the final experience among antifusion states in 1896. Both Populists and Democrats objected to accepting the others' name and, unable to fuse in the customary fashion, therefore dropped all old party names to adopt a new collective one for the purpose of the ballot: the Democratic-People's-Silver Union. But even this stratagem had a weakness, for the state supreme court ruled that by entering the DPSU the regular Democratic organization had abandoned the name "Democrat." The court therefore awarded that designation on the ballot to a ticket named by bolting anti-Bryan gold Democrats, a decision that the Democratic state chairman understandably denounced as "an attempt to mislead the people."⁴⁰

The Michigan and Ohio experiences were not lost on Republicans in Indiana, the state in which Mark Hanna feared fusion most.⁴¹ Hoosier Republicans made two unsuccessful efforts during the campaign to secure the effects of antifusion legislation without actually having such a law. When the Populists and Democrats agreed on a common electoral ticket to appear on the ballot under both the Democratic rooster and the Populist plow and hammer vignettes, the Republicans sought to enjoin the fusionists from filing dual nomination papers. Populists were alarmed that, if this tactic succeeded, "a large proportion of our voters will be practically disfranchised." One Populist, who wrote his national chairman seeking legal assistance, expressed his fear that they would "have trouble in Ind. & perhaps all other states that have accepted the Australian system of ballots to get our fusion tickets on the official ballot." The Republican state chairman also instructed Republican election judges to separate fusion votes into Democratic and Populist totals, as though the parties had different candidates. Though both of these maneuvers failed, after the election the Republicans made immediately clear, as the *Chicago Tribune* reported, their in-

³⁸ *Spokane Spokesman-Review*, August 15, 18, 22, 1896; and Winston B. Thorson, "Washington State Nominating Conventions," *Pacific Northwest Quarterly*, 35 (1944): 104-05.

³⁹ *Sioux City (Iowa) Journal*, October 17, 1896; and *Appleton's Annual Cyclopaedia*, 1896, 707-08.

⁴⁰ *Detroit Free Press*, November 1, 1896; *Kalamazoo Weekly Telegraph*, August 26, September 2, 1896; and *Baker v. Board of Election Commissioners* (Mich.), 68 N.W. Rep. 752.

⁴¹ James S. Clarkson to H. G. McMillan, October 5, 1896, James S. Clarkson Papers, Library of Congress.

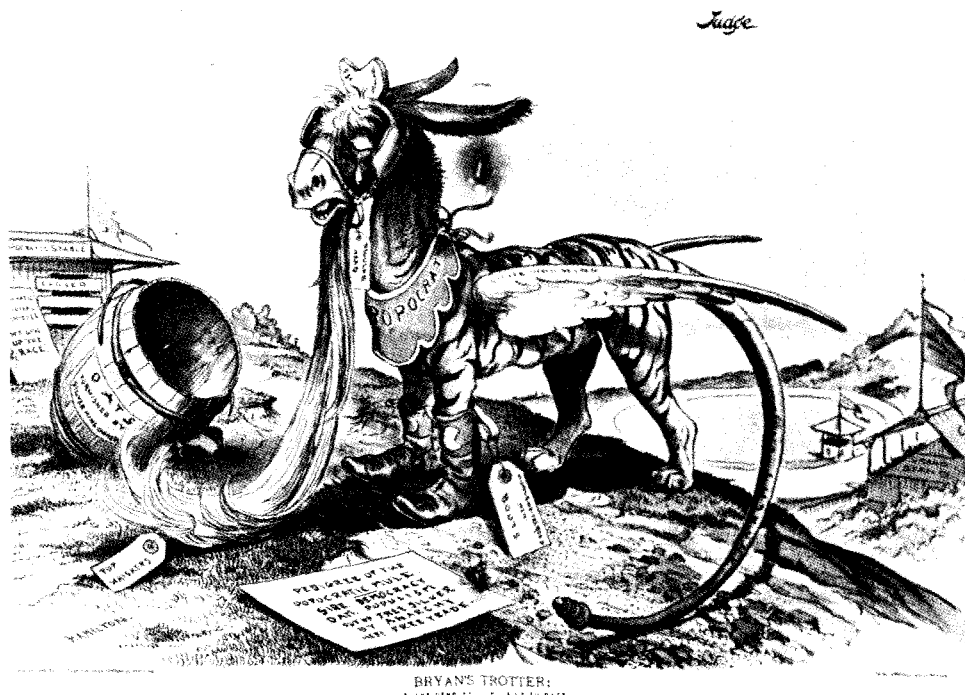


Figure 1: The “Popogriff,” a hostile depiction of the combination of political parties and factions fusing on the Bryan candidacy of 1896: the Democratic donkey with its “regular head,” Populist whiskers, middle-of-the-road (Populist) boots, Silver Party wing, Tammany tiger body, and anarchistic tail. “Pedigree of the Popocratic Mule: Sire—Democracy, Dam—Populism, Out of Free Silver by Anarchy”; *Judge*, October 24, 1896.

tention “to amend the election law, so as to prevent fusion like that perpetrated in the last campaign.”⁴² The bill, involving the customary prohibition against double listing, was drawn up by the Republican state committee and then passed in the legislature by a vote of 83 to 58, all Republicans in favor, all Democrats and Populists opposed.⁴³

The lessons learned in, and the opportunity presented by, the sweeping 1896 Republican victory led Republican-dominated legislatures in many more states to enact antifusion laws quickly. Republican legislatures passed antifusion laws in 1897 in Illinois, Iowa, North Dakota, Pennsylvania, Wisconsin, and Wyoming as well as in Indiana. As Republicans gained sufficient legislative control elsewhere, the law spread still further: California and Nebraska in 1899; Kansas, Minnesota, and South Dakota in 1901; Idaho in 1903; and Montana in 1907.⁴⁴

⁴² Lew W. Hubbell to Marion Butler, September 13, 1896, Butler Papers; *Chicago Daily Tribune*, January 4, 27, 1897; and *Cincinnati Commercial Tribune*, October 28, 29, 1896.

⁴³ *Indianapolis Journal*, January 15, 1897; *Journal of the Indiana State Senate, 1897* (Indianapolis, 1897), 592; and *Journal of the Indiana House of Representatives, 1897* (Indianapolis, 1897), 967–68.

⁴⁴ *Appleton's Annual Cyclopaedia, 1897* (New York, 1898), 395, 419, 574, 664; New York State Library Bulletin, *Summary of Legislation* (Albany, 1897), 519; *Appleton's Annual Cyclopaedia, 1901* (New York, 1902), 702; and Arthur C. Luddington, *American Ballot Laws* (Albany, 1911), 15, 39, 43, 78. South Dakota's law of 1901 followed the Populists' repeal in 1897 of the original antifusion legislation of 1893. Republican legislators had passed an antifusion bill in 1899, only to have it vetoed by the Populist governor. This pattern of ballot legislation suggests the partisan motivation involved and indicates the common conviction of the law's political effects. See

ENDING THE EFFECTIVE COOPERATION of Democrats and third party groups was both the primary goal and the major result of these efforts. As the attorney general of one state noted, the antifusion law should have been renamed "an act to keep the populists in the middle of the road." Any cooperation that did take place under the new electoral rules involved a sacrifice of voters that rendered the whole less than the sum of its parts. If forced to vote for fusion as Democrats, many Populists declared, they would prefer to return to the GOP or simply not vote at all.⁴⁵ Analysis of the Kansas election returns of 1902 confirms this. Under the 1901 antifusion law, the fusion vote declined drastically from that of 1900. Those Populists who had voted for a fusion ticket under their own heading in 1900 proved little more likely to vote for a fusion ticket under a Democratic heading in 1902 (45 percent) than actually to vote Republican (40 percent), and, when confronted with that choice, a sizable minority either voted for a symbolic third party or dropped out of the electorate altogether.⁴⁶ This last course proved even more agreeable to South Dakota Populists. The proportion of original Populists willing to support fusion fell by two-thirds between 1900, when they could vote under their own heading, and 1902, when they were required to vote as Democrats following the Republican enactment of the antifusion law in 1901. And three-fourths of that shift was accounted for by a huge increase in those who simply refused to vote at all.⁴⁷

By preventing effective fusion, antifusion laws also brought an end to another major characteristic of late nineteenth-century politics—the importance and even existence of significant third parties.⁴⁸ Whether such legislation split the

Argersinger, "Confusion to Democracy," 11. In addition, the Democratic legislatures of three Southern states also enacted antifusion legislation in the early 1900s, and controversy over the law actually provoked a riot in the Kentucky legislature. Thus, while the focus here has been on Northern Republicans, the law was obviously regarded as serving the interests of the dominant party wherever it was enacted. Antifusion legislation was of minor importance in the South because the passage of more blatantly partisan electoral legislation obviated the need for subtler controls; see Kousser, *The Shaping of Southern Politics*. Antifusion laws were more appropriate to the more closely balanced North, where slight alterations in the electorate were sufficient to guarantee partisan control. Some Northern Republicans, however, likened their antifusion legislation to the South's repressive legislation. See, for example, Des Moines *Iowa State Register*, February 19, 1897.

⁴⁵ *Detroit Evening News*, March 16, 1895; *New York Times*, April 6, 1900; and Des Moines *Farmers Tribune*, August 3, 1897.

⁴⁶ Based on an ecological regression calculated over those sixty-nine counties for which 1900 fusion votes were separately returned according to their Populist and Democratic components. This 1902 election, moreover, finally marked an approximate return to the state's pre-Populist political alignments: the Republican vote correlated significantly with the Republican vote of the 1880s for the first time in more than a decade and the Democratic vote correlated significantly with the Democratic vote of the 1880s (as the fusion votes of 1896, 1898, and 1900 had not).

⁴⁷ Based on ecological regression involving 1890, 1900, and 1902 South Dakota voting results; also see Sioux Falls (S.D.) *Argus-Leader*, November 6, 7, 8, 1902. Original Populists are here defined as those who voted the Independent ticket in 1890. Clearly, this is an incomplete measurement, for it provides no information concerning those who joined the People's Party in, say, 1894. This is only part of the difficulty in trying to measure the effect of antifusion legislation. The general question is the counterfactual one: how would things have been different if they had not been as they were? One major consequence of antifusion legislation of course could be what did *not* happen, as in those instances in which fusion was avoided. The common failure of election boards to report disaggregated partisan votes for fusion candidates, except for the partial Kansas case analyzed above, prevents a careful calculation of effects when fusion did take place. Ideally, that determination also requires consecutive fusion elections with low issue-salience, a condition that did not obtain in the 1890s. The 1892 Oregon contest thus assumes great significance in establishing the political importance of an antifusion ballot.

⁴⁸ Third parties did, of course, appear in subsequent years, but with the exception of the Socialists they were

GOP's opponents or encouraged attenuated new combinations, the same result obtained: the non-viability of third parties. A Populist explained the dynamics involved with these words: the law "practically disfranchises every citizen who does not happen to be a member of the party in power. . . . They are thus compelled to either lose their vote (as that expression is usually understood), or else unite in one organization. It would mean that there could be only two parties at one time."⁴⁹ Political realities, moreover, dictated that those two parties would be the existing major ones. Because the adoption of a new composite name left the Democratic name, with all of its appeal and tradition, to be used by minority factions as in Michigan in 1896, because of even more grotesque ballot complications under the laws of some states,⁵⁰ or merely because their greater national strength gave them an advantage in all electoral contests, the Democrats were ultimately able to insist successfully that the name "Democrat" be adopted by all fusionists. In Michigan, for example, the charade of maintaining three separate conventions ended in 1899, and in 1901 the DPSU became simply the Democratic Party. Similarly, in Washington the fusionists joined in a union convention in 1900 and agreed to the Democratic name. A Detroit newspaper alluded to this logical tendency of the antifusion law when it renamed the legislation "the law providing for the extinction and effacement of all parties but the Democratic and Republican."⁵¹

Antifusion legislation also undermined the People's Party by exacerbating the existing fratricidal split within the party between the middle-of-the-roaders and fusionists. As one Indiana Populist immediately recognized of the antifusion law, "an element of discord has been introduced by the dominant party which is expected to rend the populists' ranks and remove all doubts from future con-

generally expressive rather than instrumental. Those with any great support were short-lived and often based on the appeal of a dominant personality, like the Roosevelt Progressives of 1912 or the La Follette Progressives of 1924. Certainly, such parties rarely had, over time, the characteristics of late nineteenth-century third parties: local organization, voter identification, mass support in some areas and generalized regional strength, and especially tangible electoral success.

⁴⁹ *Kalamazoo Weekly Telegraph*, March 20, 1895.

⁵⁰ Following the passage of North Dakota's antifusion law in 1897, Independents (Populists) and Democrats, each opposing incorporation under the other's banner, combined into a new organization and adopted the title Independent-Democratic Party "as a party name . . . under which both Democrats and Populists can fight." But the imaginative Republican secretary of state interfered with this new-style fusion by ruling that the candidates of the Independent-Democratic Party could not be permitted on the Australian ballot at all, for such a party had not received the legal minimum of 5 percent of the vote in the preceding election—when, of course, it had not yet existed. Furthermore, he ruled, since the separate Independent and Democratic Parties had formed a new party, they had ceased to exist themselves and therefore could not *regain* a ballot position, leaving the Republicans the only party on the ballot. See *Winterset (Iowa) Review*, March 31, 1897; *Bismarck (N.D.) Daily Tribune*, October 28, 31, November 2, 1898; and *State v. Falley (N.D.)*, 76 N.W. Rep. 996. This action seemed to answer an earlier Populist who wondered, after the passage of an antifusion law, "why [the legislature] did not go on a little further and say there shall be but one ticket allowed on the ballot, and that must be the Republican ticket." *Des Moines Farmers Tribune*, March 17, 1897. For similar comments, see *Kalamazoo Weekly Telegraph*, March 20, 1895; and *Spokane Spokesman-Review*, August 18, 1896. Wyoming simply prevented the creation of any new-style fusions such as the DPSU or the Independent-Democratic Party by adding to its antifusion ballot amendment a requirement that the names of political parties not exceed *one* word. New York State Library Bulletin, *Summary of Legislation*, 519.

⁵¹ *Detroit Evening News*, March 20, 1895; Arthur Millsbaugh, *Party Organization and Machinery in Michigan since 1890* (Baltimore, 1917), 19, 55; *Spokane Spokesman-Review*, August 30, 1900; and *Des Moines Iowa State Register*, May 13, June 24, 1897.

tests.”⁵² The usual mid-road arguments against fusion, based on the necessity of maintaining the party’s identity and organization, acquired new and intense meaning in a legal situation that, as one judge phrased it, “says to the party, and through the party to the electors composing it: ‘You shall not endorse candidates of any other party, except on condition that you surrender your existence as a party and lose your right of representation upon the official ballot in the future.’”⁵³ Antifusion legislation thus required those Populists interested in preserving their party’s integrity to attack fusion ever more vigorously. As one Minnesota mid-roader noted, such laws would otherwise eliminate the Populists in every state where they did not outnumber the Democrats and thereby end any semblance of a national People’s Party. But fusionist Populists countered that the mid-road position, in combination with the Republican “ballot law plot,” would itself “divide and disfranchise populists and aid the monopoly and gold standard power.” They argued that third parties had had their practical importance primarily as members of fusion coalitions and that “fusion, in the manner it has been had before on the official ballot, is no longer a possibility.”⁵⁴ The logic of their position, then, required fusionists to merge the People’s Party into the Democratic ranks. Limited to a ballot choice between Democrats and Republicans, some Populists voted Republican while others dropped out. The mid-roaders, though legally a bolting minority, issued their own Populist ticket, which after lengthy court battles between the two Populist factions invariably failed to attract enough support to guarantee the party a position on the ballot in the future.⁵⁵ In either case the People’s Party ceased to exist.

IN SOME MEASURE, THEN, the People’s Party died not only from prosperity and psychic collapse but also from ballot restrictions deliberately imposed by partisan legislatures in a movement that cannot accurately be said to have “scrupulously” preserved “all the forms of political democracy.”⁵⁶ Some Populists vigorously attempted to amend the ballot laws, but others, recognizing that the Australian ballot itself had opened politics to “the dictation of state authority,” argued for its repeal. “If ‘amendment’ is insisted upon,” wrote one Iowa Populist, “let it be in the style of the farmer who amended his worthless dog’s tail by letting the cleaver fall just behind the cur’s ears.”⁵⁷ Other Populists in antifusion

⁵² Quoted in *Des Moines Farmers Tribune*, March 17, 1897.

⁵³ *State v. Anderson* (Wisc.), 76 N.W. Rep. 482.

⁵⁴ *Des Moines Farmers Tribune*, March 17, June 23, 1897. Also see O. D. Jones to Marion Butler, April 21, 1897, Butler Papers.

⁵⁵ In particular, see the Iowa experience in the *Winterset* (Iowa) *Review*, July 28, September 9, 1897; *Des Moines Farmers Tribune*, June 30, September 15, November 10, 1897; *Des Moines Iowa State Register*, August 19, 20, September 4, 8, 1897; and *Dubuque* (Iowa) *Herald*, October 17, 22, 28, 1897.

⁵⁶ Also see Burnham’s larger statement that it “is difficult to avoid the impression that while all the forms of political democracy were more or less scrupulously preserved, the functional result of the ‘system of 1896’ was the conversion of a fairly democratic regime into a rather broadly based oligarchy.” Burnham, “Changing Shape of the American Political Universe,” 23.

⁵⁷ *Sioux City* (Iowa) *Journal*, May 26, 1897; and *Des Moines Farmers Tribune*, February 17, 1897, January 5, 1898.

states began to push for electoral change to protect the existence of third parties: proportional representation.⁵⁸ That those efforts failed is hardly surprising.

Nor did the effects of antifusion legislation end with the destruction of the People's Party. Obviously, these laws contributed to the widely observed decline in party competition in the "system of 1896." It is reasonable to assume, moreover, that demoralized former Populists, whether they were forced into the Democratic or Republican Party, became more "peripheral" than "core" supporters of their new parties and were less likely to vote and, when they did, were more likely to engage in the split-ticket, drop-off, and roll-off tendencies characteristic of the electorate after the crisis of the 1890s.

Certainly, antifusion laws were not solely or even primarily responsible for those tendencies, which appeared throughout the political system. But the time has surely come to discard the notion that political effects were "unintended consequences" of nonpartisan institutional reforms. Such alterations in electoral law must be viewed within a larger political context and not treated as "uncaused causes" of the transformation of voting behavior.⁵⁹ These laws were enacted by politicians who deliberately sought to protect or advance their own interests by manipulating the rules of the game. That their interests corresponded in some respects to decreased political participation, particularly by the more democratic elements of the population, and a consequent circumscription of public policy only adds to the poignancy of the process. Obviously, it is not true that electoral "reform," as one political scientist has claimed, "ended the earlier party practice of using the institutional framework for its own benefit."⁶⁰ Indeed, antifusion laws, as one dissident observed in 1895, were "a step toward making the Australian ballot system a means for the repression instead of the expression of the will of the people."⁶¹ As for whether there was a "conspiracy," the Populists, who have often been charged with paranoia and conspiracy-mindedness, might have appreciated today's graffiti—"even paranoids have real enemies."

⁵⁸ William E. Lyons, "Populism in Pennsylvania, 1892-1901," *Pennsylvania History*, 32 (1965): 55; and *Bowling Green (Ohio) Daily Sentinel*, August 12, 1897.

⁵⁹ Burnham, "Rejoinder to 'Comments' by Converse and Rusk," 1054.

⁶⁰ Rusk, "Comment: The American Electoral Universe," 1049.

⁶¹ *Kalamazoo Weekly Telegraph*, March 20, 1895.

Cicero and Tacitus in Sixteenth-Century France

J. H. M. SALMON

IN EARLY SIXTEENTH-CENTURY FRANCE Cicero seemed a perfect model to humanists who challenged scholasticism through a rhetoric embodying both philosophy and history. Admiration for Ciceronian style was accompanied by a moralizing civic humanism and a respect for Cicero, the philosopher, as the purveyor of Greek wisdom. At the end of the century Tacitus had become a more important linguistic influence, while the ideal of the active citizen and virtuous orator had been replaced by one of Stoic fortitude and withdrawal. Tacitus, the historian of the corruption of liberty, emerged as the exemplar of private and public prudence, and a reinterpreted Cicero was relegated to the role of a minor precursor in prudential morality. This parallel shift in linguistic structures and moral ideologies did not simply result from the stresses of the religious wars in the last third of the century. It was also, in part, a long-term consequence of the earlier importation from Italy of a debate about the extent to which Cicero should be imitated—a debate that attained a new dimension in the context of the Reformation. Other elements that contributed to the demise of Ciceronian humanism were not only the relativist implications in the new historical approach to law but also the logic of Peter Ramus, who, while pleading for the union of eloquence and philosophy, effectively disjoined rhetoric from its component parts. As the neo-Stoic movement developed in reaction to rival religious enthusiasms, the literary models of Tacitus and Seneca invaded Latinity and left their mark upon the vernacular.¹ In the new climate of absolutist politics Cicero, *pater eloquentiae*, yielded place to Tacitus, *pater prudentiae*.

This article is based on a lecture delivered to the Mid-Atlantic branch of the Renaissance Society at Bryn Mawr College on October 21, 1978. My thanks are due to Felix Gilbert, Donald R. Kelley, and Arnaldo Momigliano for the helpful criticisms they made of a revised draft.

¹ There is no easy way to find logical connections between the esthetics of style and the shifting ideological assumptions of the French Renaissance. The Constance school of *Rezeptionsgeschichte und Rezeptionsästhetik* denies synchronic value and meaning to a literary work and proceeds relativistically to place it in dialectical relationship with the chain of works that preceded and followed it as well as with the interpretations of successive generations of readers. The extent to which a work alters the threshold of the reader's expectation provides the criterion for esthetic appraisal; see Hans Robert Jauss, *Kleine Apologie der ästhetischen Erfahrung* (Constance, 1972), and *Literaturgeschichte als Provokation* (Frankfurt-am-Main, 1974). As its practitioners have admitted, there are problems in applying this approach to a period when the literature of classical antiquity was regarded as an absolute standard for imitation. Nevertheless, the very intensity with which a particular classical style is defended can provoke reaction when the image of its exemplar is altered to fit the changed moral attitudes of a new age. Therefore, I will use a complementary method that traces tensions between rhetoric and

This exercise in counterpoint is not intended to explain more than a limited number of elements in an exceedingly complex movement of modes and ideas.² Yet in these terms the juxtaposition of Cicero and Tacitus seems more suggestive than the many separate studies that have related Cicero to French humanism in the first two-thirds of the century and have traced connections between Tacitism and *raison d'état* after the religious wars.³ In particular, the use that is made of both Latin authors in the interval between these two periods is important for an understanding of the shift in emphasis from one to the other. It may also be that preoccupation with particular disciplines has impeded a grasp of the broader significance of the changing Renaissance view of the classical world. Concern with the role of Cicero in humanist scholarship has been more obvious in studies of Renaissance rhetoric and philosophy than it has been in surveys of attitudes to history and politics. The reverse has been true of modern works on the influence of Tacitus, where his relationship to various strands of Machiavellism has seemed of primary importance. Moreover, it is easy to find in Livy a historical counterpart to Ciceronianism and in Seneca a philosophical counterpart to Tacitism. The apparent connections between the stylistic revolution and neo-Stoicism have been tentatively explored,⁴ and several scholars have examined the relative popularity of Livy, Polybius, Tacitus, and other classical historians during the Renaissance.⁵ What is attempted here is, on the one hand, more ambitious in its scope and, on the other, more eclectic in its exemplification.

MISCONCEPTIONS BORN FROM THE DESIRE TO SIMPLIFY readily invade discussion of literary mode. Cicero's style is by no means as monolithic as the *Ciceronianus* debate might at times suggest. The easy discursiveness of the *Epistolae ad familiares* contrasts markedly with the balanced rhythms of *De oratore*. Within the

moral philosophy and links changes in linguistic structure to shifting moral priorities. Unconscious and emotive assumptions preceding rhetorical utterance can be seen in such circumstances to affect form and content in history and political thought. Generic changes are likely to occur both in periods of political crisis and in their aftermath, when analytic thought achieves new insights and rhetoric becomes formalized. These generalizations apply to Greece in the ages of Pericles, Thucydides, and Demosthenes and to Rome in the last throes of republicanism. They have been shown to be particularly relevant to Florentine humanism in the fifteenth century; see Nancy S. Struener, *The Language of History in the Renaissance: Rhetoric and Historical Consciousness in Florentine Humanism* (Princeton, 1970). Here I have applied them to sixteenth-century France.

² Attempts to capture this complexity in full may be self-defeating; see, for example, Jean Jehasse, *La Renaissance de la critique: L'Essor de l'humanisme érudit de 1560 à 1614* (Lyon, 1976).

³ André Stegmann, "Un thème majeur du second humanisme français, 1540-1570: L'Orateur et le citoyen," in Peter Sharratt, ed., *French Renaissance Studies, 1540-1570: Humanism and the Encyclopedia* (Edinburgh, 1976), 213-33. On Tacitism, see Else-Lilly Etter, *Tacitus in der Geistesgeschichte des 16. und 17. Jahrhunderts* (Basel, 1966); Jürgen von Stackelberg, *Tacitus in der Romania* (Tübingen, 1960); André Stegmann, "Le Tacitisme," in his *Machiavellismo e Antimachiavellismo nel Cinquecento* (Florence, 1969), 117-30; Etienne Thuau, *Raison d'état et pensée politique à l'époque de Richelieu* (Paris, 1966), 32-54; and Giuseppe Toffanin, *Machiavelli e il 'Tacitismo'* (Padua, 1921). For a recent survey, wider in period than its title suggests, see Kenneth C. Schellhase, *Tacitus in Renaissance Political Thought* (Chicago, 1976).

⁴ Morris W. Croll, *Style, Rhetoric, and Rhythm*, ed. J. Max Patrick and Robert O. Evans (Princeton, 1966).

⁵ Peter Burke, "A Survey of the Popularity of Ancient Historians, 1450-1700," *History and Theory*, 2 (1966): 135-52; Arnaldo Momigliano, "Polybius' Reappearance in Western Europe," in Olivier Reverdin, ed., *Polybe* (Geneva, 1974), 347-72; and J. H. Whitfield, "Livy > Tacitus," in R. R. Bolgar, ed., *Classical Influences on European Culture, A.D. 1500-1700* (Cambridge, 1976), 281-93.

speeches themselves one should distinguish between the rich Asianism of *Pro Milone*, the middle style of Attic antithesis in *Pro Caelio*, and the “lean and energetic” vigor of the *Philippics*.⁶ Like Cicero, Tacitus knew that the style he adopted for history was quite unsuited to rhetoric. In the sixteenth century some Tacitean scholars refused to accept Tacitus as the author of *Dialogus de oratoribus* because it seemed so out of character with the Tacitus they knew.⁷ Trained as an orator in his youth, Tacitus was just as concerned about the decay of eloquence since Cicero’s age as he was about the decline of history in the sycophantic climate of imperial authority. Yet Tacitus, in his view of both oratory and history, alternated between indignation against the process of corruption and decay and acceptance of change for its own sake: Aper, one of the characters in the *Dialogus*, criticizes Cicero and argues also that all things are relative and each generation has its own fashions.⁸

Cicero’s opinions on history were even better known than those of Tacitus on rhetoric. In *De oratore* he called upon the historian to tell the truth, the whole truth, and nothing but the truth. No one was better qualified than the orator to light the lamp of the past as a guide through present darkness, and yet the style of history was not that of rhetoric.⁹ In *Brutus*, his history of Roman oratory, Cicero praised the simplicity of Caesar’s *Commentaries*: “They are bare, simple and straightforward, and devoid of all ornament. But, while Caesar wished to preserve the materials from which others might write history, he did a service for those clumsy writers who burn holes in them by trying to twist them into new shapes with curling tongs. So great is his achievement that it deterred men of good sense from writing, for there is nothing in history more agreeable than pure and shining brevity.”¹⁰ Although Jean Bodin and Montaigne cited this passage, they took less account of Cicero’s description of how other kinds of history should be written, notably the dramatic account of a single event like Salust’s *Conspiracy of Catiline* and the high-flown Asianism of later annalist technique, a form that Livy perfected. Together with Lucian’s *De scribenda historia* and the appraisal of Thucydides by Dionysius of Halicarnassus, Cicero’s remarks on history in his three mature works on rhetoric formed the *loci classici* for Renaissance scholars who sought the views of the ancients on history and its style.¹¹ Cicero wrote no history, but few were ignorant of the privileged place he ascribed to it. Without history, men would remain forever children.

Three paradoxes within the Ciceronian corpus were transmitted unrealised to the humanism of the Renaissance. First, although prudence was itself a traditional virtue, tempering the ideal to the practical demands of circumstances and

⁶ R. G. M. Nisbet, “The Speeches,” in T. A. Dorey, ed., *Cicero* (London, 1965), 47–49.

⁷ Tacitus, *Dialogue des orateurs: Dialogus de oratoribus*, ed. Henri Goelzer (Paris, 1947), 4.

⁸ *Ibid.*, xvii.

⁹ Cicero, *De oratore libri tres*, ed. Augustus S. Wilkins (Amsterdam, 1962), 2: 36 (p. 245), 62 (p. 258). Also see Pierre Boyance, *Études sur l’humanisme cicéronien* (Brussels, 1970), 136.

¹⁰ Cicero, *M. Tulli Ciceronis Brutus*, ed. A. E. Douglas (Oxford, 1966), 262, 75.

¹¹ Michel Montaigne, *Essais*, ed. Maurice Rat, 2 vols., 1 (Paris, 1962): 438; and Bodin, *Method for the Easy Comprehension of History*, trans. Beatrice Reynolds (New York, 1969), 56; and Claude-Gilbert Dubois, *La Conception de l’histoire en France au XVI^e siècle* (Paris, 1977), 72.

advocating the expedient, as Cicero did in the second book of *De officiis*,¹² weakened the morality of the common good. Second, behind the active role that Cicero for the most part commended to the citizen lay the alternative concept of the contemplative life, which pervaded his philosophical works at the end of his career and also appeared in the Stoic elements of *De officiis*, written in the same period. Third, history could not simply teach virtue if it also maintained the standards of objective truth that Cicero recommended. To later historians it was apparent from his letters to Atticus that Cicero had himself at times allowed ambition to bend the path of duty. The methods of the legal humanists involved historical relativism, and they valued Cicero's speeches and letters as sources for the science of philology and the understanding of Roman law in the particular society in which it had reached maturity. Even so, it seldom occurred to them that this society had not always lived up to Cicero's ideals.

There was a fourth area in which vision was restricted. Although, as philologists, the legal humanists were sensitive to neologisms and changes in grammatical structure, they often judged them by fixed standards of eloquence. Thus Lorenzo Valla, at once the progenitor of legal humanist method and the severest critic of Latinity, was more indignant at the barbarisms in the language of Bartolus than he was at the great commentator's lack of historicity. Yet Valla, no slavish admirer of Cicero, preferred Quintilian in matters of style, although Quintilian was himself generous in Cicero's praise. The wide popularity in France of Valla's *Elegantiae linguae latinae* contributed to the controversy aroused by the *Ciceronianus* of Erasmus.¹³ Valla felt no attraction to the style of Tacitus, who in any event was little known in the fifteenth century.

The first legal humanist to refer favorably to Tacitus in this respect was Andrea Alciato, and even he once described the historian's prose as "a thicket of thorns." Alciato published his edition of Tacitus's works in 1517, declaring him to be the best of Roman historians. Moreover, he saw in Tacitus a guide to conduct in the face of evil princes, advocating a private prudence that complemented the concept of prudence Guillaume Budé found in Cicero. For all this, it is doubtful whether Alciato, when he began to teach at Bourges in 1529, brought with him any enthusiasm for Tacitus except as an important source for the history of Roman law, and even there he made far greater use of Cicero. Alciato's mentor, Budé, who embodied all the paradoxes of civic humanism in its French guise, had no time at all for Tacitus, calling him "omnium scriptorum perditissimus" in the light of his denunciation by the Church Fathers, Orosius and Tertullian.¹⁴ Budé admired Cicero without imitating his style and

¹² In the third book of *De officiis* Cicero attempted to reconcile *utile* with *honestum*. Most modern commentators have assumed that Cicero subordinated *utile* to the common good, which is in itself a moral concept. A recent interpretation, however, reverses these priorities. Whether or not this was Cicero's intention, it was certainly seen this way by some Renaissance writers. See Maria L. Colish, "Cicero's *De Officiis* and Machiavelli's *Prince*," *Sixteenth-Century Journal*, 9 (1978): 81-93; and pages 323-24, below.

¹³ Donald R. Kelley, *Foundations of Modern Historical Scholarship* (New York, 1970), 26. Many of the Paris editions of *Elegantiae* were produced by Budé's publisher, Josse Bade. On the role of Valla's textbook in the *Ciceronianus* debate, see Augustin Renaudet, *Erasmus et l'Italie* (Geneva, 1954), 203.

¹⁴ Peter Burke, "Tacitism," in T. A. Dorey, ed., *Tacitus* (London, 1969), 149; and Schellhase, *Tacitus in Renaissance Political Thought*, 87.

felt that, in introducing Greek philosophy to the Romans, Cicero had anticipated his own vocation in respect of the French. Budé filled his *Commentarii linguae graecae* with Ciceronian citations.

In his *Institution du Prince*, addressed to Francis I, Budé used Cicero to associate rhetoric and history, observing that “the father of Latin eloquence” had described history as the “witness of time, light of truth, mistress of human life, and messenger of antiquity.” Budé recommended that the king make history his “great mistress.”¹⁵ It was a mirror in which the present reflected the past:

By its consideration men can greatly acquire prudence and better deal with affairs at hand in the counsels of princes, as we see happening every day. There is nothing more important for men of discretion than to understand the state of the world and condition of human nature and to foresee what may come to pass and how to provide for it or forestall it. This is what historians teach. Prudence is acquired in this way by the man of good judgment in reading and reflecting about the past and present government of the world and how all kingdoms and great monarchies have met their end and by what shortcomings they have fallen into difficulty or by what means they have long been preserved and maintained in power and prosperity. . . . Prudence comes for the most part from experience and from observing those examples in the past for which history serves as register.¹⁶

In the preceding passage Budé had explained how rhetoric required knowledge of history, together with “a style graceful through nature and ready invention, and the discretion and prudence to adapt what is said to the audience and the circumstances of the occasion.”¹⁷ Prudence in rhetoric was the ability to take account of the particular, and prudence in history was to understand the particularity of events and to apply to them general rules of human behavior in the interest of the public weal. It was this aspect of Cicero that was seized upon and distorted later in the century, when Ciceronian style went out of fashion and the Tacitean mode gained ascendancy. Budé represented the type of early French humanism that accepted authority in the prince and preached virtue in the citizen. Rhetoric, philosophy, and history were associated means to this end, and the works of Cicero seemed its perfect instrument. Tacitus, on the other hand, was distrusted and little known in France at this time.

THE PUBLICATION of Erasmus’s satirical dialogue, *Ciceronianus*, in 1528 exposed the tensions within the French version of civic humanism. It was not, of course, the initiation of a new controversy. Following Valla’s reservations about Cicero in fifteenth-century Italy and his exchanges on that subject with Poggio Bracciolini, Poliziano had advised the reader to choose from many models, whereas his critic, Cortesi, insisted upon the stylistic pre-eminence of Cicero. Then Giovanni Pico della Mirandola had taken a stand similar to Poliziano’s, while Barbaro had followed Cortesi. In the next generation the younger Pico, Gianfran-

¹⁵ Budé, *Institution du Prince* (1524), ed. Claude Bontems, *Le Prince dans la France des XVI^e et XVII^e siècles* (Paris, 1965), 87. This passage is a direct translation of Cicero; see his *De oratore*, 2: 36 (p. 245)—see note 9, above.

¹⁶ Budé, *Institution du Prince*, 90–91.

¹⁷ *Ibid.*, 89.

cesco, supported his uncle, and Bembo earned a reputation as the archetypal Ciceronian.¹⁸ In these debates, form and content received different priorities, and rhetoric drew apart from philosophy. The *Ciceronianus* transferred the quarrel to France at a time of religious crisis. Perhaps Erasmus, after his editing of Jerome, recalled the dream in which the saint found his Christianity at odds with his worship of Cicero.¹⁹ In his preface Erasmus turned a northern knife in an open wound. "What a loss to scholarship," he wrote, "if one began by saying that Cicero alone should be imitated. I suspect that under cover of his name another matter is at issue: I mean, to make us forget Christ and lead us back to paganism. But, for my part, no task is more necessary than to consecrate good letters to the glory of Christ, our Lord and Master." This was the same evangelism that had moved Erasmus to declare in his *Paraclesis* that the eloquence of Cicero was far less to be preferred than that of the gospel.²⁰

It seemed a gratuitous insult to couple this accusation in the preface with a slight upon the pious master of French humanism, Guillaume Budé, who twelve years earlier had extravagantly praised Erasmus's Greek New Testament to which the *Paraclesis* served as introduction. Erasmus's last letter to Budé in 1528 was a sad piece, denying any intended slur upon French learning. He wrote also to Louis de Berquin, the translator of Luther, to explain that, when the text of *Ciceronianus* had ranked the Parisian publisher, Josse Bade, before his patron Budé, the speaker had been the ridiculous arch-Ciceronian, Nosoponus, not Bulephorus, who represented Erasmus's own opinions in the dialogue.²¹ Circumstances destroyed the point of this apology. A few months later, in April 1529, the bigots of the *parlement* finally accomplished the execution of Berquin for heresy, and not long after that Budé issued his *Commentarii linguae graecae*, where Cicero, Greek philosophy, and Christian humanism were mingled without overt incongruity.

Erasmus, the editor of *De officiis* in 1507 and *Tusculanae disputationes* in 1523, did not intend to denigrate Cicero but merely to ridicule those fanatical worshippers who allowed no other standard of Latinity. While he was being denounced in France, Erasmus continued his amiable correspondence with the Italian Ciceronians, Bembo and Sadoleto, and dedicated his 1532 edition of St. Basil to the latter.²² The most vicious of his French critics was a then obscure Agenais physician named Jules-César Scaliger, whose two invectives against Erasmus, published in 1531 and 1537,²³ launched him upon his career as a scholar of international reputation. More important to the present theme was

¹⁸ Remigio Sabbadini, *Storia del Ciceronianismo* (Turin, 1885), 25–50; Eduard Norden, *Die antike Kunstprosa vom VI. Jahrhundert V. Chr. bis in die Zeit der Renaissance*, 2 (Leipzig, 1898): 773–79; and Izora Scott, *Controversies over the Imitation of Cicero as a Model for Style* (New York, 1910), 10–22.

¹⁹ M. L. Clarke, "Non Hominis Nomen sed Eloquentiae," in Dorey, *Cicero*, 87.

²⁰ Erasmus, *De recta latini graecique sermonis pronuntiatione: Dialogus Ciceronianus* (Lyon, 1531), 150; and Emile V. Telle, *L'Erasmianus sive Ciceronianus d'Etienne Dolet* (Geneva, 1974), 32.

²¹ Erasmus, *La Correspondance d'Erasmus et de Guillaume Budé*, ed. Marie-Madeleine de la Garanderie (Paris, 1967), 263–64. For the links between Erasmus and Berquin, see Margaret Mann, *Erasmus et les débuts de la Réforme française* (Paris, 1934), 113–49.

²² Renaudet, *Erasmus et l'Italie*, 218.

²³ Scaliger, *Oratio pro M. Tullio Cicerone contra Erasmus Roterodamum* (Paris [1531]), and *Adversus Des. Erasmi . . . dialogum Ciceronianum oratio secunda* (Paris, 1537).

the attack of Etienne Dolet, whose burning ambition and nonconformist temperament involved him in one feud after another. Tainted with accusations of heresy, Dolet fled from Toulouse in 1534 after a brush with the judges of the *parlement*, whom he labeled indiscriminately as “robed vultures” and “superstitious Turks.”²⁴ At Lyon he was welcomed under the sign of the phoenix and winged orb of the bookseller-printer Sébastien Gryphe, who published Dolet’s two discourses against the magistrates. Gryphe had issued a new edition of the *Ciceronianus* corrected by Erasmus in 1531 and, in his anxiety to stir the profitable pot of controversy, had just printed *Cicero relegatus*, *Cicero revocatus*, two dialogues by Ortensio Landi, an Italian exile in France. Dolet had in his possession the manuscript of his *Commentarii linguae latinae*, a Ciceronian Latin counterpart to Budé’s introduction to Greek letters. While Dolet went to Paris to obtain a privilege for his masterpiece (arriving there in the midst of the storm created by the posting of extremist Protestant placards), Gryphe began to set up the text of a new reply to Erasmus adapted from several entries in the *Commentarii*. It appeared in 1535 as *Dialogus de imitatione Ciceroniana adversus Desiderium Erasmum Roterodamum pro Christophoro Longolio*.²⁵

Longolius, or Longueil, was a Ciceronian protégé of Bembo much revered by Dolet. A new and posthumous edition of his works in 1526 may have provoked Erasmus to compose the *Ciceronianus*. His published letters had accused the sage of Rotterdam of Lutheranism and compared his scholarship unfavorably with that of Budé. Assuming Longueil to be the model for Nosoponus, Dolet inserted into his own dialogue Longueil’s friend and Dolet’s master at Padua, Simon de Neufville. Through the mouths of Villanovanus (Neufville) and his foil Morus (a deliberate travesty of Erasmus’s friend, Sir Thomas More, then awaiting execution in the Tower of London), Dolet vilified the character and works of Erasmus. After the latter’s death Dolet also responded to an Italian defender of the *Ciceronianus*, Floridus Sabinus, who, oddly enough, had been secretary to Alberto Pio, prince of Carpi, the anti-Erasmian. There was little but personal abuse in this polemic coupled with praise of Budé and Poliziano and a note of respect for the shade of Erasmus.²⁶

Dolet published his reply to Sabinus on the press he had recently established at Lyon, and in the following year, 1541, he issued his own edition of Valla’s *Elegantiae*. It was almost as if Dolet, beset by foes among humanists of both camps as well as defenders of scholastic method, was trying to shelter beneath past authority. Scaliger pursued him with implacable hatred. Led by Jean Visagier and Gilbert Ducher, the circle of Latin poets also united against him, including Hubert Sussannée, the author of a Ciceronian dictionary, and Antonio de Gouvea, who later opposed the Ciceronianism of Ramus. Nor did Dolet find support from the legal humanists, with whom in his early years he had shown an affiliation. Jean de Boyssoné, the disciple of Budé and friend of Alciato who

²⁴ Marc Chassigne, *Etienne Dolet* (Paris, 1930), 56.

²⁵ Telle, *L’Erasmianus sive Ciceronianus d’Etienne Dolet*, 40–42.

²⁶ *Ibid.*, 35–36; and Dolet, *Stephani Doleti Galli Aurelli liber de imitatione Ciceroniana adversus Floridum Sabinum* (Lyon, 1540), 7, 54. Dolet apologized for calling Erasmus “delirans senex batavus.”

had been expelled from his chair of Roman law because of accusations of heresy, was probably his protector at Toulouse. In his letters to Boyssoné, Dolet had denounced the Bartolists, and he had once written to Budé expressing the hope that he could study under Alciato.²⁷ But Alciato came to profess impatience with Valla's literary preoccupations, and Dolet in his last years sided with Valla, symbolizing the growing divorce between literary and legal humanism. While Alciato became more concerned with history than with philology and used literary texts purely to exemplify aspects of Roman society, Dolet turned into a grammarian who subordinated historical truth to literary excellence and Christian to pagan inspiration.

Dolet's device on the title page of the works he published as a printer in Lyon consisted of a hand holding an axe to trim the branches of a fallen tree. The punning motto read, "Scabra et impolita adamussim Dolo atgue perpolio" ("accurately, do I hack and level rough and unpolished pieces"). His axe sought to lop off French humanism from its evangelical stem. Among the many classical works that issued from his press were Cicero's *Tusculans* and *Rhetorica*. As stories circulated about his unbelief, he put tongue in cheek and supported his vigorous denials by printing a book as bizarre as *Cato Christianus*, an attempt, dedicated to Sadoletto, to explain Christian doctrine with the aid of an archpagan moral reformer. Dolet also began to translate into French Cicero's *Epistolae ad familiares*, a task that, when completed by the historian Belleforest in 1565, was accompanied by the latter's warning to the reader that his predecessor had been "un imitateur trop sévère." Dolet's French edition of *Tusculans* contains a preface to Francis I pleading that his fellow printers in Lyon were falsely accusing him of heresy in order to eliminate a successful competitor. Nonetheless, Dolet's version of the text in which Cicero questioned the immortality of the soul in this same volume contains added ambiguity. Here, too, are remarks about Roman use of prudence in government and Greek association of prudence and rhetoric.²⁸ Dolet's attitude to Cicero suggests that Erasmus's reproach of paganism was not entirely wide of its mark. His involvement in the *Ciceronianus* debate reveals the tensions within the French humanism of the time. Erratic, perverse, and eternally enigmatic, Dolet met his cruel death in the Place Maubert in 1546, but it is not only for his martyrdom that he deserves to be remembered.

The year of Dolet's execution was a turning point in Ciceronian scholarship for another reason. In 1546 Pierre de la Ramée (Ramus) published *De studiis philosophiae et eloquentiae conjugendis*, his commentary on *Brutus* (*Brutinae quaestiones*), and *Dialecticae institutiones*, which bears the name on the title page of Omer Talon, Ramus's acolyte.²⁹ Ramus was in search of a single logical method that he hoped to apply to all of the disciplines of the liberal arts. Some of his

²⁷ Lucien Febvre, *Le Problème de l'incroyance au XVI^e siècle: La Religion de Rabelais* (1942; Paris, 1968), 51–61; and Chassigne, *Etienne Dolet*, 23–29.

²⁸ Cicero, *Les Epistres familières de Marc Tulle Ciceron pere d'eloquence . . . traduites de Latin en Francois . . .*, trans. Dolet and F. de Belleforest (Paris, 1566), unpaginated advertisement au lecteur, and *Les Questions tusculanes de M. T. Ciceron: Oeuvre tres utile et necessaire pour resister à toute vitiueuse passion d'esprit, et parvenir au mespris et contennement de la mort* (Lyon, 1543), 2–20.

²⁹ Walter J. Ong, *Ramus—Method and the Decay of Dialogue: From the Art of Discourse to the Art of Reason* (Cambridge, Mass., 1958), 30.

inspiration was derived from Rodolphus Agricola's *De inventione dialectica*, a fifteenth-century treatise on the logic of disputation aimed at scholasticism. But, ultimately, Ramus's system probably stemmed from Cicero's remarks in *Brutus* on the traditional division of rhetoric into five parts: *inventio* (discovery of material or general arguments), *dispositio* (arrangement), *elocutio* (style), *pronuntiatio* or *actio* (delivery), and *memoria* (memory).³⁰

The new method consisted essentially in transferring the first two elements to logic and using them as a means of proceeding from the general to the specific by way of bifurcatory analysis. Only style and delivery remained to rhetoric, while recollection or narration dropped from consideration. Although Ramus might pretend to conjoin philosophy and rhetoric, and rhetoric appeared to be his starting point, he advanced to a position in which dialectical logic became the queen of the sciences and rhetoric was reduced to a matter of technique. Moving logic and dialectic back to philosophy thus recreated an essential element in the attitudes of the schoolmen. These changes reversed the priorities that the Ciceronian humanists held dear—for rhetoric was no longer the dominant discipline that could gather others beneath its roof—and Ramus accomplished it all in Cicero's name. In so doing, he changed the entire concept of the union of citizen and orator. As Ramus gradually refined his method, he altered his stance from edition to edition of his works until the dialectical system came to consist of four parts: invention, disposition, distribution, and definition (or reconstruction). So fixed in its final form did this structure become that in his lecture *De logicae definitione* Ramus declared that, whatever the part of the world in which men dwelt, whatever their customs and governments, and whatever the divisions of knowledge they chose to admit (*grammatici, rhetores, poetae, historici, arithmetici, geometrici, musici, astrologi, physici, ethici*), all were obliged to follow "the one law and reason postulated in the dialectic."³¹

Ramus's dramatic flair and arrogant single-mindedness provoked intense opposition from fellow Ciceronians and all of the followers of Aristotle, whom Ramus had denounced. The resistance within the Ciceronian movement to Dolet's attempt to isolate letters from other humane studies was carried over into the struggle to prevent Ramus from imposing upon them a method supposedly derived from Ciceronian premises. Antonio de Gouvea, one of Dolet's critics, played the major part in the commission that resulted in the royal edict of March 1544, which censured Ramus for "témérité, arrogance, et impudence." While officially suspended from public teaching in the years that followed, Ramus continued to compose works on Cicero and Quintilian that exemplified his system, including many commentaries on Cicero's speeches and philosophical treatises. After the advent of Henry II in 1547, the suspension was lifted, and Ramus, with strong support in the royal entourage, made new converts in the *parlement* and the university. His appointment as joint professor of eloquence and philosophy at the Collège Royal in 1551 enabled him to initiate institutional re-

³⁰ Ramus, *Brutinae quaestiones*, in his *Petri Rami praelectiones in Ciceronis orationes octo consulares*, ed. Freigius (Basel, 1575), 472. For a list of Cicero's references to the five divisions in *Brutus*, see Douglas, Introduction to *Brutus*, xxxi.

³¹ Ramus, *Scholae in liberales artes* (1569), ed. Walter J. Ong (Hildesheim, 1970), 30.

forms of the liberal arts and provoked new waves of opposition. Pierre Galland, the conservative rector of the university and an early critic of the *Brutinae quaestiones*, led the outcry, describing Ramus as “a harpy that fouls all that he touches” and “a viper vomiting forth floods of poison.”³² Galland was followed by Ramus’s most relentless enemy, Jacques Charpentier, who, together with his friend, Marc-Antoine Muret, criticized the *Dialecticae institutiones* and resisted the union of rhetoric and philosophy.

Unlike Dolet, Ramus was careful not to mix religious speculation with his commentaries on Cicero’s philosophical works. When he lectured on the *Somnium Scipionis* and hinted, darkly enough, that the rest of Cicero’s *Republic* was doubtless kept well hidden by superstitious men, he used the occasion not to discuss problems of the afterlife but to apply his practical method to physical science. The very nomenclature used in astronomy, he said, was the product of the scholastic mind and suited only for “altercationes scholasticae.” But Ramus’s colleague, Adrien Turnèbe, did not object to metaphysical speculation in his criticism of the logician’s commentaries on Cicero’s *De fato* and *De legibus* but rather to the distortions involved in the application of Ramus’s interpretative system. In this context Turnèbe branded Ramus with the epithet *usuarius*, meaning that he had exploited Cicero to illustrate his method and had no proprietary claim to the Ciceronian canon. Ramus seemed close to asserting such a demand. After rereading the *Brutus* on famous orators, he published in 1557 a number of his lectures under the title *Ciceronianus*. Here he identified himself with the father of eloquence and was so contemptuous of earlier controversies and commentaries that he labeled them “patchwork quilts of rags” (*panniculorum centones*).³³

In the religious wars of the 1560s Ramus, a declared if irregular Calvinist, fled from Paris, leaving the field to his enemies. Meanwhile, Denis Lambin, successor to Turnèbe as professor of Greek at the Collège Royal in 1566, proceeded with his edition of Cicero’s *Opera omnia* in unyielding opposition to Ramist influence. Henri Estienne, whose revised version of the Ciceronian lexicon composed by his father, Robert, was completed by Lambin’s index in 1568, returned to the tradition of Poliziano and Erasmus and satirized Mario Nizzoli, the leader of the old-style Italian Ciceronians. Estienne also drew attention to the rising star of Justus Lipsius, who with Marc-Antoine Muret, Charpentier’s friend, inaugurated the age of Tacitean scholarship. Even Ramus, for whom Estienne had no sympathy, anticipated Lipsius in some respects. His angular Latin style resembled that of the great antiquarian, and his view of the virtue of

³² Charles Waddington, *Ramus (Pierre de la Ramée): Sa vie, ses écrits, et ses opinions* (Paris, 1885), 50–52, 94. For a list of Ramus’s commentaries, see Walter J. Ong, *Ramus and Talon Inventory* (Cambridge, Mass., 1958), 38. Several of these commentaries, together with *Brutinae quaestiones* and *De studii philosophiae et eloquentiae conjugendis*, were published in 1575 in the posthumous collection edited by Freigius, Ramus’s disciple at Fribourg; see note 30, above.

³³ Ramus, *Somnium Scipionis ex sexto libro de republica M. Tullii Ciceronis Petri Rami Veromandue praelectionibus explicatum* (Paris, 1546), 26v, *Ad Turnebi disputatio ad librum Ciceronis de fato adversus quemdam qui non solum logicus esse, verumetiam dialecticus haberi vult* (Paris, 1554), 27, and *P. Rami regii eloquentiae et philosophiae professoris Ciceronianus ad Carolum Lotharingum Cardinalem* (Paris, 1557), unpaginated dedication. For an account of the exchanges between Turnèbe and Ramus, see Ong, *Ramus and Talon Inventory*, 289–94.

prudence in the orator touched, as Budé had formerly done, upon a theme that Lipsius later expanded.³⁴ Ramus met his death in the massacre of St. Bartholomew's Day—legend has it by Charpentier's conniving—while Estienne continued a stylistic debate that became increasingly irrelevant. In the religious wars the burden of intellectual debate shifted from rhetoric and philosophy to politics and history, and Tacitus suddenly assumed a new dimension.

THE NEW TONE WAS EXPLICIT in Jean Bodin's *Methodus ad facilem cognitionem historiarum* in 1566: "It is true that on account of his unpolished manner of speaking Tacitus is usually repudiated by those who prefer the lighter trifles of grammarians to the more serious accounts of those who have spent the whole of their lives in public affairs." Bodin reproached Alciato for quibbling about the style of Tacitus and remarked that those who criticized Tacitus for plain writing were like Christians attacking Jerome for being a Ciceronian. In contrast to Ammianus Marcellinus, Tacitus had "maintained the dignity of Roman speech." He ranked with the great historians of antiquity—Polybius, Thucydides, Xenophon, and Caesar—for like them he revealed "the causes of things, the origins, the progress, the inclinations, all the plans of everyone, the sayings and the deeds at their just weight." Influenced by Ramus and disillusioned with legal humanist attitudes, Bodin set out in his *Methodus* to deduce the principles of government from a comparativist approach to history. If he differed from Budé in this respect, he repeated Budé's observation that for men of affairs history was the essential means of acquiring political prudence. Cicero, however, did not appeal to Bodin. He accused Cicero of being digressive, refuted his belief in a government of mixed forms, and decried the republican and democratic values he discerned in Cicero's political thinking. "Cicero's definition of the state," he wrote, "as a group of men associated for the sake of living well indicates the best objective indeed, but it does not indicate the power and nature of the institution." He also assailed another Ciceronian account of the commonwealth as "the union of several associations under an approved law for a common advantage." Bodin's preference for strong monarchy led him to distort Tacitus's acceptance of the principate as the necessary consequence of the decay of republican virtue, and to see the historian as approving undivided authority by preference: "It is not only salutary, as Tacitus wrote, but also necessary in the administration of great affairs that power should rest entirely with one man."³⁵

Another admirer of Tacitus, and one who described him as "a good author, and as serious and reliable as any,"³⁶ was Montaigne's friend Etienne de la Boétie. His *Discours de la servitude volontaire* was completed about 1550 on the model of a classical indictment of tyranny, but it was not published until after the St. Bartholomew's Day Massacre, when it was pressed into the service of

³⁴ Jehasse, *La Renaissance de la critique*, 189–90, 330; Dubois, *Conception de l'histoire en France*, 250; and Ong, *Ramus—Method and the Decay of Dialogue*, 246. Henri Estienne's *Pseudo-Cicero* appeared in 1577, and his *Nizoliodasculus* in 1578; see Scott, *Controversies over the Imitation of Cicero*, 105.

³⁵ Bodin, *Method for the Easy Comprehension of History*, 70, 83, 73, 17, 158, 272.

³⁶ La Boétie, *Discours de la servitude volontaire* (1574), ed. Paul Bonnefon (Paris, 1922), 83.

Huguenot propaganda against the monarchy.³⁷ Although he esteemed Tacitus as much as Bodin, La Boétie interpreted him quite differently and cited him in condemnation of the many crimes of the Julio-Claudian dynasty. Interestingly enough, the passage where he particularly praised the merits of the historian was as scathing of the *plebs sordida* as it was of Nero, who had so corrupted them that they mourned his death. A more celebrated work that used Tacitus against monarchical absolutism was François Hotman's *Francogallia*, composed largely in the late 1560s and first published in 1573. Yet among the greatest achievements of Hotman's early career were his massive textual commentaries on the orations and letters of Cicero. His major collection of observations upon Cicero's forensic pleadings had appeared as early as 1554. The stature of his scholarship in this respect may be judged from the pride of place given his comments in the Cologne compendium of Cicero's speeches in 1685 and his remarks on the *Epistolae ad Brutum et ad Quintum fratrem* in the composite edition of Gronovius in 1725.³⁸ Consciously or unconsciously, the political demands of the civil wars bent the objectivity of scholars. Hotman made selective use of both Cicero and Tacitus as allies in his cause.

The French situation resembled that in Germany at the beginning of the century, when humanists such as Conrad Celtis and Ulrich von Hutten enlisted Tacitus's *Germania* in the cause of patriotism and resurrected Arminius from the darkness of the Teutoberger Wald. The Alsatian humanist Beatus Rhenanus had imposed a more scholarly view with his edition of Tacitus's works in 1532, which surpassed even Alciato's in accuracy.³⁹ Now the French began to examine Tacitus for the light he might shed upon the political practices of their ancestors, the Franks, and such relevance as they might have for the sixteenth-century constitution. Although Hotman had made his name as a historian of Roman law, he came to see the tribal customs of the Germanic Franks as more important to Frenchmen than Roman jurisprudence. When Bodin was writing his *Methodus* in the interval between the civil wars of the 1560s, Hotman was preparing both his *Anti-Tribonian* and, with the help of the Beatus edition of Tacitus, his early outline of the history of the Francogallic constitution. The *Germania* was used by other historians and polemicists writing in this period, such as Etienne Pasquier and Jean du Tillet, although their views of French government were very different from Hotman's.⁴⁰ Their arguments were complicated by a debate about whether the Franks had been Teutons or migrant Celts and about the relative merits of Gaulois and Frankish customs. When these controversies were initiated in the previous decade Ramus had been a leading Gallicist and Charles du Moulin an early advocate of Germanism.⁴¹ Bodin and Hotman, respectively, continued these opposing traditions.

³⁷ The *Discours* first appeared in the second of the *Dialogi ab Eusebio Philadelphio* (or *Reveille-Matin*) in 1574 and was reprinted in the various editions of Simon Goulart's *Mémoires de l'estat de France sous Charles Neufiesme* (1576–79).

³⁸ Hotman, *Francogallia* by François Hotman (1573), ed. Ralph E. Giesey and J. H. M. Salmon (Cambridge, 1972), 38–52; and Cicero, *Marci Tullii Ciceronis orationum commentaria selecta* (Cologne, 1685), and *M. Tullii Ciceronis epistolarum ad Quintum fratrem et ad Brutum* (The Hague, 1725).

³⁹ Schellhase, *Tacitus in Renaissance Political Thought*, 37–47, 61–65.

⁴⁰ Hotman, *Francogallia*, 22.

⁴¹ *Ibid.*, 13; and Ramus, *Liber de moribus veterum Gallorum* (Paris, 1559).

When the *Francogallia* appeared in 1573 and went through massive revision and expansion in subsequent editions, the scholarly intentions of the draft begun in the 1560s took second place to the political needs of the later period. Tacitus and Cicero were invoked in tandem: the first to reveal the primitive Franks as the authors of liberty who elected their kings *ex nobilitate* and chose their war chiefs *ex virtute*; the second to extoll the merits of mixed government and repeatedly to stress the doctrine *salus populi suprema lex esto*, taken by Cicero from the Twelve Tables and described in *De legibus*.⁴² For all his Ciceronian scholarship, Hotman overlooked a vital reference that would have helped his case, the mention in *Epistolae ad Atticum* of the German Francones. The anti-quarian Claude Fauchet took pride in pointing this out. He agreed with Hotman on the possible identification of the Franks with the Sicambrians of Batavia, but, more sympathetic to monarchical authority, he held the references of Tacitus to Germanic tribal kingship to be less restrictive on royal power than Hotman had claimed.⁴³

Other Huguenot theorists of resistance made less use of Tacitus than did Hotman, but they frequently cited Cicero. In *Du droit des magistrats* Théodore de Bèze referred to a passage that Seneca had quoted from the missing books of *De republica* to the effect that even under the early Roman kings final appeals could be made to popular assemblies. Similar use of this example was made in *Francogallia*.⁴⁴ Bèze drew the attention of his readers to a section describing tyranny in Cicero's *De officiis*, where it was held that in the last resort it was the duty of a son to accuse a father who threatened the safety of the state through tyranny. The author of *Vindiciae contra tyrannos* cited *De officiis* on many occasions to demonstrate that those in authority had to protect true religion, respect the law, and act for the welfare of the people who had placed them in office. As in *Du droit des magistrats* Cicero was invoked to greatest effect in defining the nature of tyranny. Rulers who betrayed their trust or broke their contract with those who had created them to govern became enemies of the people, committed treason against the commonwealth, and ought to be punished.⁴⁵ The *Discours politiques des diverses puissances*, a Huguenot tract rather less known than *Francogallia*, *Du droit des magistrats*, or *Vindiciae contra tyrannos*, seems to have been inspired by Ciceronian concepts in its attempt to adapt principles of civic humanism to secular resistance theory. Once again, *De officiis* was the work most often cited in the text.⁴⁶

⁴² Hotman, *Francogallia*, 208, 294, 296, 300, 342, 414, 450.

⁴³ Fauchet, *Oeuvres*, 1 (Paris, 1590): 29v, 30r.

⁴⁴ Bèze, *Du droit des magistrats* (1574), ed. Robert M. Kingdon (Geneva, 1971), 25; and Hotman, *Francogallia*, 300.

⁴⁵ Bèze, *Du droit des magistrats*, 47–48; and Cicero, *De officiis*, ed. H. A. Holden (Cambridge, 1899), 3: 23, 90 (p. 127). And [Mornay] *Vindiciae contra tyrannos* . . . *Stephano Iunio Bruto Celta Auctore* (n.p., 1580), 28–29, 75, 105, 109, 121–22, 156, 179, 212, 215–16. Only one Ciceronian allusion in the *Vindiciae contra tyrannos* was not drawn from *De officiis* but came from Cicero's *Paradoxa Stoicorum* instead; *ibid.*, 179.

⁴⁶ *Discours politiques des diverses puissances establies de Dieu au monde* (1574) in Goulart's *Mémoires de l'estat de France sous Charles Neufmesme*, 3 ("Meidelberg," 1579): 147v, 213r. For some definitions of tyranny from Cicero often employed in resistance theory, see *ibid.*, 165r, 167r (references to *Second Philippics*), 168v (*De officiis*, 1: 8, 26 [p. 12]), 177v (the passage from *De officiis* that Bèze cited in *Du droit des magistrats*; see *De officiis*, 3: 23, 90 [p. 127]; and note 45, above). The author also referred to Tacitus. I am indebted to Sarah H. Madden for drawing my attention to the Ciceronian tenor of this important and neglected tract.

Apart from Hotman, Huguenot writers generally confined their use of Tacitus to historical exemplification.⁴⁷ Cicero seemed an appropriate source for the definition of tyranny and Tacitus for its illustration. Those of moderate persuasion, who supported both royal authority and religious toleration, were even more inclined to see Tacitus's *Annales* and *Historiae* as useful sourcebooks, especially when they found themselves opposing alleged flatterers and Machiavellians around the throne. Such was the case with the Protestant magistrate Innocent Gentillet, who, while he was no monarchomach, shared the xenophobic Huguenot view that the crown had been corrupted by Italian influence in general and Machiavelli in particular. Gentillet's *Discours contre Machiavel* made much of flattery, deceit, and treason under Tiberius from Tacitus's *Annales*. He quoted Tacitus's version of the speech of the jurist Gaius Ateius Capito that the emperor should not pardon the technical treason of Lucius Ennius because the "laws desire that in such crimes of treason the least suspicion and appearance suffice to convict the accused, and it is in the great interest and utility of the state that one who has attempted anything, however slight, against the ruler should be rigorously punished."⁴⁸ Capito's words came close to genuinely Machiavellian reasoning, but Gentillet, who was so accustomed to distorting Machiavelli's precepts that he could not recognize a true instance of *raison d'état* when he saw it, was concerned only to criticize a piece of hypocritical flattery. Gentillet never accused Tacitus of being a precursor of Machiavelli. The Roman historian could still be seen in La Boétie's image of the exposé of tyrants, not the exponent of tyranny.

Jean Bodin, as we have seen, was an admirer of Tacitus not merely as a historian of causes and motives but also as an advocate of royal authority. His *Six livres de la République*, published in the same year as the *Discours contre Machiavel*, was also critical of Machiavellians and was aimed, besides, at the Huguenot theorists of resistance who themselves condemned the Florentine. At the beginning of his new book Bodin defined the commonwealth as "a lawful government of several families and what they hold in common, together with sovereign power." This sounded reminiscent of the definitions cited from Cicero in the *Methodus*, but the addition of sovereignty made all the difference in the world. With sovereignty described as absolute, with perpetual and indivisible law-making power located in the French crown, Bodin went one better than the monarchomach collective sovereignty of the people, exercised negatively to restrain or discipline the king.⁴⁹ Like Gentillet, Bodin saw no resemblance between Tacitus and Machiavelli, and he, too, used passages from the *Annales* without recognizing an affiliation with *The Prince*. Tiberius was the subject of several critical references in *Six livres de la République*, while Bodin alluded to Sejanus as an example of the useful royal technique of diverting discontent toward a ty-

⁴⁷ For example, see [Mornay] *Vindiciae contra tyrannos*, 141; and *Discours politiques*, 165r, 182v.

⁴⁸ Gentillet, *Discours contre Machiavel* (1576), ed. A. d'Ancree and P. D. Stewart (Florence, 1974), 77; and Tacitus, *Annales*, 3: 70. For other relevant allusions to Tacitus, see Gentillet, *Discours contre Machiavel*, 98, 101, 102, 163, 197.

⁴⁹ J. H. M. Salmon, "Bodin and the Monarchomachs," in Horst Denzer, ed., *Verhandlungen der internationalen Bodin Tagung* (Munich, 1973), 359–78.

rannical minister.⁵⁰ In this passage Bodin did not hesitate to call Tiberius himself a tyrant, but he went on to cite Cicero, first, on the futility of abolishing the good with the bad in attempting to erase a tyrant's memory after his death and, second, on the dilemma confronting a member of a tyrant's council during the consideration of some profitable law. Tacitus was also called in on many occasions in *Six livres de la République* to sustain erudite references to such miscellaneous issues as Roman paternal authority, the chastity of the ancient Germans, and the Parthian custom of swearing alliances in blood. Except for a new rebuttal of the theory of mixed forms of government, Cicero was also employed more as a source of historical illustration than of evaluative judgment. His letters and speeches, together with *De officiis* and *De finibus*, were cited over thirty times on matters such as the family, citizenship, usury, and the role of magistrates.⁵¹ Bodin was too conscious of his own originality to require authorities to buttress his theoretical viewpoint. The architect of *politique* absolutism used Cicero and Tacitus indiscriminately as a grab-bag of information.

Bodin chose to write *Six livres de la République* in French, although he turned it into Latin in 1586, and the issues of Latin eloquence and literary style were irrelevant to his treatise. As more emphasis in political dialogue was placed upon the vernacular, a number of French translations of Tacitus and Cicero began to appear. Mode could still be important, however, to a translator. In 1575 Blaise de Vigenère published a composite volume in French containing part of Tacitus's *Germania*, the sixth book of Caesar's *Commentaries*, and Cicero's *De oratore*.⁵² His declared purpose in placing these three works between one set of covers was to show how a diversity of Latin styles could be faithfully represented in French. Vigenère, who had studied under Turnèbe and Dorat, also translated Livy and the dialogues on friendship of Plato, Cicero, and Lucian. Claude Guillemot published a complete French version of *Germania* in 1580, but it had evidently been prepared at the beginning of the Gaulois-Germanist controversy, for its dedication was dated 1551. The *Agricola* was translated in 1574 by the Huguenot lawyer Ange Cappel, who also rendered several of Seneca's works into French.⁵³ In 1582 Fauchet, the antiquarian, issued a complete translation of the

⁵⁰ Bodin, *Les Six livres de la République* (Geneva, 1577), 2: 5 (p. 392). In another passage Bodin did not hesitate to follow Tacitus in observing that Tiberius took away from the people "l'ombre de la liberté"; *ibid.*, 1: 6 (p. 83).

⁵¹ For Tacitus, see Bodin, *Six livres de la République*, 1: 4 (p. 47), 8 (p. 174), 5: 1 (p. 776); and, for Cicero, see *ibid.*, 1: 4 (p. 47), 2: 1 (pp. 338–39), 5: 2 (p. 802), 3: 5 (pp. 522–27).

⁵² Vigenère, comp., *Le Traicté de Cicéron de la meilleure forme d'orateurs, le sixième livre des commentaires de Caesar . . . , et la Germanie de Cornelius Tacitus* (Paris, 1575). After serving the duke of Nevers, Vigenère was employed in the French embassy in Rome and then in the office of one of the secretaries of state. He was a prolific author, and among his more curious works are treatises on comets, alchemy, and writing in cipher. Also see his translations of Plato et al., *Trois dialogues de l'amitié: Le Lysis de Platon—le Laelius de Cicéron—et le Toxaris de Lucian* (Paris, 1579), and of Livy, *Les Décades*, 2 vols. (Paris, 1583).

⁵³ Tacitus, *Discours excellent auquel est contenu l'assiete de toute l'Allemagne, les moeurs, coustumes, et façon de faire les habitants en icelle*, trans. Claude Guillemot (Paris, 1580), and *La Vie de Jules Agricola*, trans. Ange Cappel (n.p., n.d. [dedication dated 1574]). Cappel's brother Guillaume wrote a poem for the *Agricola* in honor of Ange and addressed it to Theodore de Bèze. Guillaume, who joined the Catholic League, was also a translator of Machiavelli. Another brother, Louis, was a professor of theology at the new University of Leyden, where he was a colleague of Lipsius. A fourth brother, Jacques, published a work in defense of Bartolus against Valla. Ange Cappel was an ardent reformer of legal procedures and a confidant of Sully. Descended from an eminent magistrate in the *parlement* of Paris, this extraordinary family seems to represent all of the intellectual threads incorporated in this paper.

works of Tacitus, incorporating parts of the *Annales* that Etienne de la Planche had already turned into French.⁵⁴ Among the more remarkable translators of Cicero at this time was the strange figure of Guy le Fèvre de la Boderie, remembered for his Syriac New Testament, his Chaldean grammar, his refutation of Islam, and his translations of Ficino and Giovanni Pico. Not surprisingly, he chose to publish Cicero's *De natura deorum* in French in 1581.⁵⁵

WHILE TACITUS WAS INCREASING IN POPULARITY among French readers and the theorists of resistance, together with their critics, managed to set him beside Cicero in support of their contentions, the two greatest Taciteans of the century—Marc-Antoine Muret and Joost Lips, Muretus and Lipsius—were laying the foundations for the triumph of their idol. Both were concerned with elegance and accuracy in Latin style; both began as Ciceronians and, after changing the focus of their attention to Tacitus, returned to Cicero with new insight; and both found the times in which Tacitus had lived a better analogue for the troubles the monarchies of Europe were experiencing in their own day than was the age of Cicero. It was in part because of their joint perception that a new literary mode was to be accompanied by a shift in the mode of political discourse.

As a youth Muret was an admirer of Erasmus's critic Jules-César Scaliger. His erudition and the brilliance of his own Latin style secured him lectureships at several French universities, including Paris, where he was a member of Charpentier's anti-Ramist group, and Bordeaux, where he may have tutored Montaigne. Charges of heresy and sodomy caused him to take refuge in Italy. At Venice in 1557 he dedicated to the doge a commentary on Cicero's speeches against Catiline, and adopted so ultra-Ciceronian a stance that he addressed Mocenigo, "It is extraordinary to see how these men, who in our own and our father's memory wanted to be called Ciceronians, could come out with so many barbarous expressions and defective constructions taken from the corrupt texts they consulted."⁵⁶ Typical of Muret's academic showmanship was his laying a trap for his rivals when they attended his lectures in the hope he would utter a phrase not in Cicero's canon. He put careful stress on a number of words that he knew to be contained in Cicero's works but that had been inadvertently omitted from the great lexicon prepared by Nizzoli. Muret was a professor in Rome for twenty-one years, until shortly before his death in 1585. When he began his series of lectures on Tacitus in 1580 his enthusiasm for his subject was so great

⁵⁴ Tacitus, *Les Oeuvres de C. Cornelius Tacitus*, trans. Claude Fauchet (Paris, 1582). *Le Dialogue des orateurs* was added to the edition of 1584–85. A third edition was published in 1594. Tacitus's *Annales* also entered France through Guicciardini's *Storia d'Italia*, which was deeply influenced by them. French editions of Guicciardini's history were published in 1568 and 1577; see Vincenzo Luciani, *Francesco Guicciardini e la fortuna dell'opera sua* (Florence, 1949), 32, 398. Guicciardini's *Ricordi*, containing allusions to Tacitus, appeared in French in 1576; see Guicciardini, *Plusieurs avis et conseils de François Guicciardin tant pour les affaires d'estat que privées* (Paris, 1576). I am indebted to Lionel A. McKenzie for this point.

⁵⁵ Cicero, *De la nature des dieux*, trans. Guy le Fèvre de la Boderie (Paris, 1581). In his Oriental interests and his religious syncretism La Boderie was a follower of Guillaume Postel.

⁵⁶ Muret, *M. Antonii Mureti ad Leonardum Mocenicum Patricium Venetum, orationum Ciceronis in Catalinam explicatio* (Venice, 1557), iii.

that he declared history to be the queen of the humanities.⁵⁷ He had a utilitarian history in mind, and Tacitus was its high priest. The lessons that Tacitus taught applied to the current age. While Muret was cautious enough to declare that Europe had no contemporary tyrants to compare with Tiberius, Caligula, and Nero as described in the *Annales*, he believed "it profitable for us to know how good and prudent men managed their lives under them, how and how far they tolerated and dissimulated their vices. . . . Those who do not know how to connive at such things not only bring themselves into danger but often make the very princes worse."⁵⁸ When the lectures on Tacitus had been completed, Muret turned back to Cicero and used *Epistolae ad Atticum* as his text. The Cicero that now emerged from the confidences imparted to his closest friend was a devious statesman in whom ambition vied with prudence. The only use for eloquence in modern times, Muret concluded, was in the writing of letters.⁵⁹

There is no space here to follow in detail the academic peregrinations of Lipsius from Jena to Leyden to Louvain or his religious apostasies among Lutheranism, Calvinism, and Catholicism. He was first known not only as a Ciceronian scholar but also as a supreme Ciceronian stylist. In later years he recalled his youthful enthusiasm and continuing, if modified, affection for Cicero in a piece of Latin that is almost a parody of the Tacitean mode: "Ciceronem amo. Olim etiam imitatus sum: alius mihi sensus nunc viro. Asianae dapes non ad meum gustum, Atticae dapes."⁶⁰ But, if Lipsius altered his estimate of Cicero's style, he remained consistent in his belief that Cicero's principal merit in politics was the advocacy of prudence. This was the virtue he commended when dedicating his first lectures on Cicero to Cardinal Granvelle before setting out for Rome in 1567.⁶¹ In Rome he met Muret and began his career of Tacitean scholarship, in which the concept of prudence developed into a kind of short-hand for political necessity, guarded by the moral reservations of the individual.

The two aspects of prudence, the private and the public, and the two fields of its exemplification, history and rhetoric, had been left disjoined by Budé. Now public and private prudence were directly associated, and politics replaced rhetoric as the second area in which prudence could be learned and applied. The connections had already been implicit in Cicero and the Stoic context of the orator's thought. The intellectual climate of the religious wars in France and the Netherlands had at first encouraged the application of Ciceronian rhetoric to political debate, but as the wars intensified the Ciceronian emphasis upon the active participation of the citizen no longer seemed appropriate. The help-

⁵⁷ Muret, *M. Antonii Mureti opera omnia*, 1 (Leyden, 1789): iii. For Muret's showmanship, also see J. E. Sandys, *A History of Classical Scholarship* (Cambridge, 1908), 150.

⁵⁸ Croll, *Style, Rhetoric, and Rhythm*, 152.

⁵⁹ Muret, *Mureti opera omnia*, oratio XVI, 318-25. Despite his new view of Cicero and his contempt for ultra-Ciceronians, Muret paid tribute to Sadoletto, Bembo, and Longueil in this lecture.

⁶⁰ "I love Cicero. Formerly I even imitated him. Now as a man I have other tastes. Asian feasts are not to my liking. Attic ones are." Lipsius, as quoted in Clarke, "Non Hominis Nomen sed Eloquentiae," 95. For brief outlines of Lipsius's life, together with comments upon his ideas, see V. A. Nordman, "Justus Lipsius als Geschichtsforscher und Geschichtslehrer," *Suomalainen tiedeakatemia Toimituksia (Annales Academiae Fennicae)*, ser. B, 28 (1932): 1-101; Jason Lewis Saunders, *Justus Lipsius: The Philosophy of Renaissance Stoicism* (New York, 1955); and Knud Banning, *Justus Lipsius* (Copenhagen, 1975).

⁶¹ Jehasse, *La Renaissance de la critique*, 208. Lipsius cited Cicero and Tacitus together on prudence; see Lipsius, *Politicorum sive civilis doctrinae libri sex* (Leyden, 1589), 212.

lessness of the individual in face of forces he could not control suggested resignation or withdrawal, and the tenets of Senecan neo-Stoicism provided the moral fortitude that enabled him to preserve his dignity and integrity. In this way neo-Stoicism conjoined the two kinds of prudence, encouraged the politics of necessity, and elevated history as the record of pragmatic realism and the source of individual consolation. The transformed Cicero still had a role to play in this new structure, but among classical authors Tacitus now occupied the center of the stage. Lipsius personified this intellectual shift and popularized the change in literary mode through which it was expressed. In France he had his counterparts or disciples in Montaigne, Guillaume du Vair, and Pierre Charron.

Despite his desire for scholarly withdrawal, Lipsius could not ignore the blood bath in his own country and across the borders to the south. When he lectured on Tacitus at Jena in the year of the St. Bartholomew's Day Massacre, he compared the Duke of Alba with Tiberius.⁶² Three years later appeared the first version of his masterly edition of the works of Tacitus, which he refined and improved in subsequent editions until his death in 1606. Also published in 1575, Lipsius's *Antiquae lectiones* confirmed the revision of his view of Cicero, although here he turned to Plautus and Seneca, rather than to Tacitus, for literary models. In the next decade came his neo-Stoic *De constantia* (1584) and his political handbook based extensively upon Tacitus, *Politiorum libri sex* (1589). A statement of expediency in the latter work—that the toleration of two religions inside one state was inconceivable and that one of them must be exterminated *ense et igne*—caused indignation at Leyden but was evidently ignored by Calvinists at La Rochelle, who printed a French translation there in 1590.⁶³ Lipsius continued to publish studies of Roman antiquities and earned equal rank with Isaac Casaubon and Joseph Scaliger in the scholarly “triumvirate” of the age, though Casaubon and Scaliger did not see eye to eye with Lipsius in his role of *Sospitator Taciti*. Casaubon denounced Tacitus as a specialist in shame and slighted his commentator by saying that anyone who thought Tacitus a guide to practical politics was either accusing kings of being tyrants or teaching them how to become so. Scaliger, with an acerbity reminiscent of his father, wrote that “Lipsius is the cause that many have now little respect for Cicero, whose style he esteems about as much as I do his own.” Yet in literary matters Lipsius became something of a relativist. One of his most readable pieces was a satire, published in 1581, in which he described a dream where he witnessed the Roman senate prosecuting a group of audacious philologists who sought to correct ancient texts by their own standards.⁶⁴

⁶² Arnaldo Momigliano, “The First Political Commentary on Tacitus,” in his *Essays in Ancient and Modern Historiography* (Oxford, 1977), 222–24; this essay was first published in the *Journal of Roman Studies*, 37 (1947): 91–101. The later version includes as an appendix a reprint of Momigliano's review of Jose Ruyschaert, *Juste Lipse et les Annales de Tacite* (Louvain, 1949), itself a valuable work on the editorial methods of Lipsius. In the review Momigliano discussed whether the oration in question was actually delivered by Lipsius at Jena in 1572 and pointed out that Lipsius also remarked in the oration that Tacitus was more relevant to his own times than were Sallust and Livy. Also see Jehasse, *La Renaissance de la critique*, 213.

⁶³ Lipsius, *Les six livres des politiques, ou Doctrine civile de Justus Lipsius*, trans. Charles le Ber sieur de Malassis (La Rochelle, 1590).

⁶⁴ Jehasse, *La Renaissance de la critique*, 394–96; Saunders, *Justus Lipsius*, 65; and J. Ijsewijn, “Neo-Latin Satire: *Sermo* and *Satyra Menippeae*,” in Bolgar, *Classical Influences on European Culture, A.D. 1500–1700*, 49.

BEFORE THE NEW CURRENT had become firmly established in France a curious echo of that phase of Huguenot resistance theory that had enrolled Cicero and Tacitus together under its banners sounded in the propaganda of the Catholic League. The most weighty ideological opponent of the last Valois and the first Bourbon kings was Jean Boucher, doctor of the Sorbonne and associate of the revolutionary group of the Sixteen. His *De justa Henrici Tertii abdicatione* appeared in 1589, just after the monarch it sought to depose had been assassinated by a Leaguer fanatic. To illustrate the nature of tyranny Boucher cited Tacitus's judgment of Sejanus together with some of the extracts from *De officiis*, *Pro Milone*, *Ad Brutum*, and *Tusculanae disputationes* that formed the corpus of humanist sources on the subject. From *De amicitia* he repeated Cicero's words on the climate of tyranny: "Under it there can be no trust, no love, no kind of enduring good will: suspicion and unease lurk everywhere, and there is no place for friendship."⁶⁵ This, Boucher maintained, was precisely that atmosphere surrounding Henry of Valois and his would-be successor, Henry of Navarre. Boucher's pseudonymous colleague, "Gulielmus Rossaeus," published a better reasoned and less personal treatise to the same effect: *De justa reipublicae Christianae in reges impios et haereticos* (1590). Rossaeus cited one of Cicero's letters to Atticus and, a little later, the second book of *De officiis*, to establish the difference between a king and a tyrant. Each reference to Cicero was associated with a quotation from Tacitus on the reign of Tiberius.⁶⁶ Another enemy of Navarre was the Leaguer publicist and *parlementaire* Louis Dorléans, whose humanist inclinations had been shaped by Dorat and whose first polemics for the League were written in the guise of "the Catholic Englishman." Like Boucher, Dorléans fled to the Netherlands when Henry IV recovered Paris in 1594. He returned in later years after making his submission, and he eventually issued his *Novae cogitationes in libros Annalium Taciti*. Here Tacitus was represented not as the critic of imperial tyranny but as the ultimate guide to prudent kingship. During his exile Dorléans had evidently become a disciple of Lipsius. Formerly, he argued, it was customary "to praise the declamation of Cicero, but Tacitus is more deserving in our age." To Dorléans Tacitus had become "a flower among authors and a prince among historians."⁶⁷

The first separate political commentary on Tacitus was published in Paris in 1581, three years before Navarre had become the next heir to the French throne and the League had been revived to oppose his claim. It was issued with the text of the first four books of the *Annales*, and its author was a Piedmontese, Carlo

⁶⁵ [Jean Boucher] *De iusta Henrici Tertii abdicatione e Francorum regno* (1589; Lyon, 1591), 258 (mispaginated as 266).

⁶⁶ Rossaeus, *De iusta reipub. Christianae in reges impios et haereticos auctoritate* (Paris, 1590), 17r, 18v, 22r, 22v. On the identity of Rossaeus, who used the pseudonym only in the 1592 Antwerp edition of the work, see J. H. M. Salmon, *Society in Crisis: France in the Sixteenth Century* (London, 1975), 274; and Frederic J. Baumgartner, *Radical Reactionaries: The Political Thought of the French Catholic League* (Geneva, 1975), 145–47. The use by Rossaeus of Cicero's *De officiis* is very like that of the French monarchomachs; see pages 318–19, above. He is probably directly indebted, however, to the Scottish monarchomach, George Buchanan, to whom he referred on several occasions with surprising respect. Rossaeus's citation from *De officiis* is the same as Buchanan's: Buchanan, *De iure regni apud Scotos* (1579; n.p., 1680), 28. And see Cicero, *De officiis*, 2: 12, 41 (p. 75).

⁶⁷ Dorléans *Novae cogitationes in libros Annalium Cornelii Taciti qui extant* (Paris, 1622), unpaginated dedication.

Pasquale.⁶⁸ He was a protégé of the statesman and man of letters, Guy du Faur de Pibrac, a widely respected popularizer of Seneca. Pasquale turned to Seneca in his preface to the reader when he wished to commend those who, as Tacitus had shown, knew how to respond to unexpected adversity and even to death. He used Seneca, too, to defend the enigmatic brevity of the historian's style, remarking that language responded to changes in public attitudes.⁶⁹ Lipsius was popularizing gnomic political utterances on the Tacitean model, and when Pasquale republished his commentary in 1600 he chose to retitile it *Gnomae seu axiomata politica ex Tacito*.⁷⁰

No single writer better illustrates the shift in moral attitudes, the new mode of discourse that displaced humanist rhetoric, and the altered relationship between Cicero and Tacitus than does Montaigne in the first two books of his *Essais*, which were published in 1580, and then in their revised form, together with the third book, in 1588. Montaigne developed his own early Stoicism through Seneca and Plutarch quite independently of Lipsius, and only after Lipsius praised his essays in 1583 did they begin to correspond. Indeed, apart from two mentions of Lipsius in the third book, Montaigne's acknowledged and unacknowledged debts to *Sospitator Taciti* were included only in the revisions of the essays made after 1588.⁷¹ By his own account Montaigne first paid close attention to Tacitus soon after the time that Bodin praised the historian in the *Methodus*. Nevertheless, most of the references to Tacitus in the essays of 1580, including even the passage on the death of Seneca, were indirectly acquired through Bodin's work and other sources. Montaigne was in Rome when Muret lectured on Tacitus in the winter of 1580–81. He reread Tacitus when composing the third book, and he offered his most important judgments upon him in "De l'art de conférer." He noted how Tacitus revealed the effects that the cruelty of the emperors produced upon their subjects, and declared that this kind of material offered more insights than standard accounts of battles and high matters of state: "This type of history is by far the most useful: public affairs depend more upon the whim of fortune, private ones upon our own conduct." Montaigne found more precepts and judgments than narration in Tacitus: "It is a seed-bed of ethical and political discourses for the use and ornament of those who have status in the management of the world."⁷² As Bodin and Muret had done, the essayist defended Tacitus for his adherence to the state religion. Like Lipsius, Montaigne held it to be the duty of a citizen to follow the

⁶⁸ Momigliano, "The First Political Commentary on Tacitus," 205–29.

⁶⁹ Pasquale, "Genus dicendi, ut ait Seneca, imitatur publicos mores," in Tacitus, *C. Cornelii Taciti Equitis Romani ab excessu Divi Augusti Annalium libri quatuor priores et in hos observationes Caroli Paschali cuneatis* (Paris, 1581), unpaginated preface to the reader.

⁷⁰ See Lipsius's own collection of Tacitean and other political axioms, *Iusti Lipsii monita et exempla politica libri duo, qui virtutes et vitia principum spectant* (Paris, 1605). Once again, *civilis prudentia* is represented as the governing concept in this volume.

⁷¹ The section that follows is heavily dependent on Pierre Villey's *Sources et l'évolution des Essais de Montaigne*, vol. 1: *Les Sources et la chronologie des essais* (Paris, 1908), 224–26 (Tacitus), 98–103 (Cicero), 214–16 (Seneca), 198–99 (Plutarch), 161–62 (Lipsius).

⁷² Montaigne, *Essais*, 2: 377–78 (3: 8). (Book and chapter numbers in these and subsequent references to Montaigne appear in parentheses.)

religion established by law, and, while he was more tolerant toward the individual conscience than was the scholar of Leyden, he agreed with Lipsius that lack of religious uniformity was an invitation to civil strife.

Montaigne developed a strong interest in the personality of Tacitus, and offered a few criticisms, largely in the revisions for the 1595 posthumous edition of the essays. Tacitus was said to conceal himself too much from the reader and even from himself. His judgment of individuals was occasionally at fault, and there were times when he wrongly preferred the rules of civility to frankness and truth. Nevertheless, he was "a great personage, upright and courageous, endowed not with a superstitious nature but philosophical and generous." The value of his history was never in doubt. No one else had managed to blend consideration of manners, attitudes, and particular motives with a record of public events. His opinions were sound, and he had taken the right side in Roman politics. His role had been "proper in a sick and troubled state, as ours is in the present age."⁷³

As it might be expected from Montaigne's humanist background, the essays abounded in references to Cicero, but, on nearly every occasion where Montaigne offered personal judgments in the versions of 1580 and 1588, they were of a negative sort. Montaigne admitted that Cicero deserved his reputation for eloquence, but that was not the author's concern in the essays. His main interest lay in moral ideas, and Cicero's were not to his taste. He made considerable use of such works as *De amicitia*, *De senectute*, and *De finibus*, but he apparently did not study Cicero's speeches or his treatises on rhetoric; Montaigne's references to such works were stock ones borrowed from other sources, including the *Dialogus de oratoribus* of Tacitus. As a man, Montaigne found Cicero vain and untrustworthy. His harshest comments, however, were reserved for Cicero's style, which he described as "lasche et ennuyeuse."⁷⁴ Cicero's work was filled with prefaces, definitions, and digressions without reaching the substance of his discourse. One could read him for an hour and come up with no more than a bag of wind. Montaigne despised both the subtleties of the grammarians and the logic-chopping of the Aristotelians. Cicero, he declared, might be suitable for the schoolroom or the bar—or, indeed, to the kind of sermon where one could go to sleep for fifteen minutes and awake to find the same thread of argument. Cicero's artifice negated the point he wished to make. If Montaigne wanted to hear a speech on love of liberty, he preferred Brutus to Cicero, and, if he wished to cultivate disdain for death, he read Seneca before Cicero.⁷⁵

After 1588, however, Montaigne's view of Cicero changed in the last respect. His developing interest in metaphysical questions led him to see Seneca and Cicero as complementary, and he made considerable use of Cicero's philosophical works, especially *Quaestiones academicae*, *Tusculanae disputationes*, *De natura deorum*, *De divinatione*, *De officiis*, and *De finibus*. Much of the scepticism of the *Apologie pour*

⁷³ *Ibid.*, 380, 378 (3: 8).

⁷⁴ *Ibid.*, 1: 454 (2: 10). (The 1595 version omits *lasche*.)

⁷⁵ *Ibid.*, 1: 455 (2: 10), 2: 121 (2: 31).

Raymond Sebond had dropped away, and Montaigne renewed his earlier interest in the tempered and resigned Stoicism of Seneca. There were aspects of Cicero, especially in *De finibus*, which could easily be reconciled with this stance, but Montaigne never varied his view of Cicero's rhetoric. Like Pasquale, he remembered what Seneca had written on the adaptation of linguistic structure to the concerns and moral conventions of the age. His own opinions resembled the moral and political attitudes of Seneca and Tacitus, and his mode of discourse in the vernacular was the counterpart of the new Latinity of Lipsius.

Guillaume du Vair shared the attitudes of Montaigne and Lipsius on the inefficacy of the individual to affect the capricious twists of fortune, on the need for a resolute demeanour in personal conduct, and on the disastrous political consequences of religious innovation. He served in the *parlement* of the Catholic League, where his patriotism and his dislike of the revolutionary element in the Catholic faction moved him to oppose the suggestion that the Spanish Infanta should become queen of France. Du Vair composed his treatise on constancy during Henry IV's siege of Paris in 1590 and published it when the king finally entered his capital four years later. His work was inspired by Seneca and, more immediately, by Lipsius. Du Vair had written on Epictetus and on Stoic moral philosophy in general, but he was also an admirer of Cicero, whom he adapted to fit the neo-Stoic mood. The future keeper of the seals was the author of *De l'éloquence française*, where he provided Cicero's *Pro Milone* as an example for emulation, and explained the decay of French rhetoric in terms of the loss of the old Ciceronian ideal of the union of the orator and the citizen. Despite his lament about present evils and the powerlessness of the individual to hold back the tide, Du Vair continued to plead for the active life. In this vein he wrote *Exhortation à la vie civile*, which became attached to his tract on constancy. Moreover, he was an advocate of political prudence who despised a priori political theorists: "They want to reduce political government, which depends upon particular prudence, to the general rules of some universal science, and in applying rules in this way where they ought to be applying exceptions they have distorted judgment in everything."⁷⁶

Editions of Cicero were still being produced and read, based upon the Aldine versions, especially that of Paulus Manutius, and those of French scholars such as Charles Estienne and Denis Lambin.⁷⁷ Yet even in the editorial process a change was evident. In 1588 the jurist Denis Godefroy published at Lyon a revised text of Cicero's *Opera omnia* as prepared by Lambin. In his preface Godefroy criticized his predecessor for being too concerned with trivialities of language at the expense not only of philosophical issues but also of practical matters of politics and individual conduct. "I have always thought," he wrote, "that we should rather pay attention to what concerns the *mores* of our own

⁷⁶ Du Vair, *Traité de la constance et consolation es calamitez publiques*, ed. Jacques Flach and F. Funck-Bretano (Paris, 1915), 201. The other works mentioned are Du Vair's *De l'éloquence française* (Paris, 1590) and *La Sainte philosophie, la philosophie des Stoïques, manuel d'Epictète, civile conversation, et plusieurs autres traictez de pieté* (Lyon, 1600). On Du Vair in general, see R. Radouant, *Guillaume du Vair: L'Homme et l'orateur* (Paris, 1909); and L. Zanta, *La Renaissance du Stoïcisme au XVI^e siècle* (Paris, 1914).

⁷⁷ Villey, *Sources et l'évolution des Essais de Montaigne*, 102–03.

country, family life, and government, than wear away childishly this fleeting, brief, and miserable life in wrestling with the forms of letters: it is more important to live well and wisely than to learn to speak too scrupulously and exactly." In consequence, Godefroy omitted many of Lambin's philological notes. Godefroy had been trained at Louvain as a Ramist and had later become a Calvinist. Immediately after editing Cicero, in his Genevan place of exile, he set to work on the textual emendation of Seneca's philosophical works, not by the comparison of manuscripts but rather by conjecture. Like his edition of the *Corpus juris civilis*, his edition of Cicero went through many reprintings in later generations. It was dedicated to Elector Frederick William IV, who became Godefroy's patron. He was invited by the jurist to pay less attention to examples of Cicero's elegance than to Cicero's practical precepts, which should be applied to government and the reformation of public morals.⁷⁸

Times were indeed changing. Montaigne and Pasquier both remarked that Ciceronian eloquence could only apply where the vulgar had power: it was irrelevant in France. The *Satyre Ménippée*, the Politique riposte to the quasi-democratic propaganda of the league, had its most serious spokesman, Daubray, refer to a page of Tacitean history and to Tacitus himself as "an evangelist to some."⁷⁹ There was a hint here that Tacitus could be a source of amoral counsel, which was also the view of Giovanni Botero, a Savoyard diplomat who had visited Paris in 1585. Botero's *Della Ragion di Stato* (1589) was the first work directly to associate Tacitus with Machiavelli, and it was published in French translation in 1599.⁸⁰ Such an opinion long remained uncommon in France. The Huguenot poet and historian, Théodore-Agrippe d'Aubigné, who upheld an intensely moral position in his later years, described Tacitus as *mon maître*.⁸¹ Scipione Ammirato invoked Tacitus to refute Machiavelli's republicanism and composed his *Discorsi sopra Cornelio Tacito* (1594) as a counter to Machiavelli's *Discourses on Livy*. Ammirato saw Tacitus as the supreme advocate of political prudence. Jean Baudouin, who provided a new French version of the works of Tacitus, also translated Ammirato's *Discorsi*. Evidently he did not expect his readers to be shocked by the Italian's personal explanation: "If I gave here precepts and lessons on how to dissimulate, it is sufficient for me to record the words of holy scripture where it is said that even God dissimulates the sins of men so that they may be converted."⁸²

Even if Machiavelli was still regarded with some suspicion in France, political prudence, soon to be linked with reason of state,⁸³ was becoming an accepted

⁷⁸ Cicero, *M. Tullii Ciceronis opera omnia, praeter hactenus vulgatam Dionysii Lambini editionem, accesserunt D. Gothofredi*, 1 (Lyon, 1588): unpaginated dedication and preface to the reader.

⁷⁹ *Ménippée*, 1 (Ratisbon, 1726): 146–47. On the background of the *Satyre Ménippée*, see J. H. M. Salmon, "French Satire in the Late Sixteenth Century," *Sixteenth-Century Journal*, 6 (1975): 57–88.

⁸⁰ Schellhase, *Tacitus in Renaissance Political Thought*, 124.

⁸¹ Thuau, *Raison d'état et pensée politique à l'époque de Richelieu*, 35; and Tacitus, *Les Oeuvres de C. Corn. Tacitus . . . avec des discours politiques* by Ammirato, ed. and trans. Jean Baudouin (Paris, 1628), *Discours* (separately paginated), 10. On Ammirato, see Schellhase, *Tacitus in Renaissance Political Thought*, 142–45.

⁸² Botero, *Raison et gouvernement d'état* (Paris, 1599).

⁸³ On the affiliation between such terms as prudence, interest, and reason of state, see Anna Maria Battista, "Morale 'privée' et utilitarisme politique en France au XVII^e siècle," in Roman Schnur, ed., *Staatsräson: Studien*

part of the ideology of monarchical absolutism. Pierre Charron, the disciple of Montaigne, represents the completion of the shift in moral philosophy. The third book of Charron's *De la sagesse* discussed "the four moral virtues of prudence, justice, strength, and temperance." Strength (*force*) had been substituted for the traditional fortitude, and prudence had replaced wisdom. Prudence, indeed, had become "the general queen, superintendent, and guide of all the other virtues," and Cicero was cited as the interpreter of the two complementary kinds, private and public.⁸⁴ In actuality Tacitus and Seneca dictated the manner in which Charron interpreted Cicero. They were cited together to demonstrate that sovereignty required equity in its exercise and should not be confused with a tyranny in which the prince was above the law. Yet the world was full of malicious men whose treachery was wont to subvert the state unless the prince mixed prudence with justice and composed his cloak of both the pelt of the fox and the skin of the lion. It was in this context that one acted for the public welfare and cited the Ciceronian maxim *salus populi suprema lex esto*.⁸⁵

Cicero had counseled the statesman to be watchful, to believe nothing and guard against all eventualities, and Seneca had taught the necessity of dissembling. As Tacitus had indicated in *Agricola*, the prince must keep himself informed of all that happened, must never relax, and must constantly employ dissimulation. Many references of this kind played fast and loose with the texts they purported to cite, and the names of Tacitus, Seneca, and Cicero in the margins of Charron's book did not necessarily imply a just reflection of what these authors had actually written. Tiberius and Vespasian joined the ranks of "great princes and wise statesmen" with Augustus, Trajan, Hadrian, and the Antonines. Tacitus had taught that evil princes were the instrument of God's justice and must be suffered, and when Cicero had cited Greek opinions about tyrants he had referred to those who had usurped authority, not to legitimate princes. Seneca, for his part, had instructed the counselor of rulers to trim his sails to fit the wind, while Tacitus had taught him discretion not to pry into royal secrets that should be kept hidden.⁸⁶ Piety, honor, and equity were words that Charron employed at intervals, but they, like everything else, were twisted to conform with the new linguistic mode in which wisdom had become the slave of prudence.

IT WOULD BE EASY TO ASSUME that the linguistic and ideological shift evident in the alternating fortunes of Cicero and Tacitus was merely the by-product of the religious strife and social dislocation experienced in sixteenth-century France. Certainly, the personal lives of those whose allusions to Cicero and Tacitus have

zur Geschichte eines politischen Begriffs (Berlin, 1975), 87–119; and J. H. M. Salmon, "Rohan and Interest of State," in *ibid.*, 121–40.

⁸⁴ Charron, *De la sagesse: Trois livres* (1601; Paris, 1604), 477–78, 482. On Charron in general, see J. B. Sabrié, *Pierre Charron: De l'humanisme au rationalisme* (Paris, 1913); and H. Busson, *Le Rationalisme dans la littérature française de la Renaissance, 1533–1601* (Paris, 1957).

⁸⁵ Charron, *De la sagesse*, 488–89. The image of the lion and the fox was not necessarily from Machiavelli: Charron could have found it in *De officiis*; see Cicero, *De officiis*, 1: 13, 41 (p. 17).

⁸⁶ Charron, *De la sagesse*, 497, 526, 504.

been traced in these pages were profoundly affected by persecution and civil war. Dolet and Ramus met violent deaths. Muret and Lipsius accommodated their outward creed to the places in which they found refuge. Among the opponents of absolutism, Hotman and Bèze were intimately concerned with the conduct of the Protestant cause, and Boucher and Dorléans with that of the Catholic League. Those whose opinions were in some sense *Politique*—Gentillet, Bodin, Du Vair, and even Montaigne—were to a greater or lesser degree involved in negotiations or administrative responsibilities in time of civil faction and religious conflict. Undeniably, the emergence of Tacitean prudence and the reinterpretation of Cicero owed much to the triumph of *Politique* compromise and the mutual exhaustion of Catholic and Protestant enthusiasts.

Yet the wave of humanist culture in the early part of the century and the determination of its Epigoni to give it practical expression in the higher levels of the royal administration contained undercurrents of intellectual change, whatever the political environment. Ciceronian rhetoric embraced too many disciplines. The debates among the philologists, the dialecticians, and the defenders of eloquence for its own sake destroyed the unity of humanist endeavor and fostered the advance of new theories of politics and history. This was the malaise after the reign of Francis I, which Bodin recalled as “the bitterest sense of grief that those brilliant flashes of talent which shone throughout all France have been extinguished in desolation and want.”⁸⁷ Ultimately for this reason Montaigne came to profess his contempt for the grammarians and the logicians. It was also true that war and fanaticism had caused the humanists either to appear irrelevant or to adapt their moral and political ideas to inappropriate ends. Disillusionment resulted in the triumph of the least virtuous of the virtues. Prudence, the art of particularity and dissimulation, emerged in both its public and its private aspects, the one to shield the arcana of the absolutist state, the other to shelter the citizen in the fortress of the individual mind. Attitudes to Cicero and Tacitus in sixteenth-century France reflected the destruction of that humanist ideal whereby a highly educated elite might participate in an enlightened administration. State and society stood in antithesis to one another, and the French variety of Renaissance humanism disappeared from intellectual life.

⁸⁷ Bodin, *Method for the Easy Comprehension of History*, 7.

Incumbered Wealth: Landed Indebtedness in Post-Famine Ireland

L. P. CURTIS, JR.

“Your income from lands is so cut up by interest of money,
annuities to poor relations and to fosterers, so that,
in the best of times, you have but little to *actually touch*.”

“An Irish estate is like a sponge,
and an Irish landlord is never so sick as when he is sick of his property.”¹

OVER THE YEARS IRISH LANDLORDS have had a rather poor press. Writers of both history and fiction have depicted them as personifying almost all of the seven deadly sins with few, if any, redeeming virtues. Such hallowed works in the nationalist repertoire as Michael Davitt's *The Fall of Feudalism in Ireland*, Seumas MacManus's *The Story of the Irish Race*, and Dorothy Macardle's *The Irish Republic* contain angry or bitter allusions to this “alien class” that stole the land from the people and then spent the next three centuries in pursuit of pleasure at the expense of the “native Gael.”² A recent best-selling historical romance dealing with the struggle for independence, *Trinity*, reinforces the familiar stereotype of the slow-witted (the wits dulled by drink or drugs) and rapacious landlord draining the countryside of capital in the form of rackrents and giving nothing in return except “evictions, rent raises, and public floggings.”³ The political attractions of this view make it unlikely that the scholarly studies of landlord-tenant relations and landlordism by W. A. Maguire, Barbara L. Solow, James S.

Research for this article was supported over the years by the American Council of Learned Societies, the American Philosophical Society, and the National Endowment for the Humanities. The author would like to thank these foundations as well as the numerous archivists and librarians whose cooperation brought this project to fruition. For permission to inspect documents and to publish from them special thanks are due to the Representative Church Body, to the Records Branch of the Irish Land Commission, to the Manuscripts Department of the National Library of Ireland, and to the Public Record Office of Northern Ireland.

¹ James O'Connell to Daniel O'Connell, March 28, 1822; and the Earl of Dufferin to the Duke of Argyll, May 7, 1874.

² For Davitt's indictment of landlordism, see *The Fall of Feudalism in Ireland* (London, 1904), esp. xv-xviii, 26-33. MacManus accused “the absentee landlord” (as if there were no resident landlords) of having evicted thousands of tenants from their holdings; see MacManus, *The Story of the Irish Race* (rev. ed., New York, 1967), 603, 606. In her lengthy account of the Irish struggle for independence, Macardle claimed that “until the end of the [nineteenth] century the ‘hostile army’ of landlords was helping to empty Ireland of its inhabitants while in neighbouring countries populations multiplied”; Macardle, *The Irish Republic* (London, 1968), 43.

³ Leon Uris's sketch of the wicked father of Morris Hubble, Earl of Foyle, perpetuates the familiar stereotype: “The old Earl passed on, basking in the Indies in the arms of an extremely young, beautiful, black mistress. He had lived in supreme style and died from an unmentionable disease, the pain of which had been removed by a heavenly veil of opium.” Uris, *Trinity* (New York, 1977), 70-71. For other glimpses of Uris's views of Irish landlordism, see *ibid.*, 45, 126-30.

Donnelly, Jr., and William E. Vaughan will do much to alter, much less improve, the notorious reputation of the landlord—at least in Irish or Irish-American eyes.⁴

Central to the popular image of Irish landlords has been their acquisitive instinct, their seemingly insatiable appetite for wealth. To judge from nineteenth-century Irish novels, the country was filled with reckless or impecunious members of the gentry who spent money as fast as they could beg, borrow, or marry it. Indeed, circumstantial evidence suggests that the Irish landed elite devoted much of its private life to money cares, which meant in practice searching for low-interest loans and dodging bill collectors. To argue for an objective as well as analytical approach to Irish landlordism does not mean that evicting or rack-renting landlords should be ignored, but professional historians, unlike the makers of myths and partisans of earlier generations, have to concern themselves with questions that go beyond the pursuit of landlord “villains.”⁵ Such historians must chart the behavior of a whole range of landlords who cannot simply be put into categories marked “good” and “bad.” In Irish historical literature it is all too easy to find tales of landlord avarice or iniquity. What is rare is documentary evidence covering the whole history of an estate from its creation in the centuries of conquest and confiscation to its liquidation through sale in the years after 1885. Because histories of landed estates vary as much as the lives of the people who lived on them, it is not unusual to find estates in the same barony undergoing different experiences during good times or bad.

Irish estates varied greatly in size, resources, credit, and management practices, and generalizations about their operations often founder on exceptions so numerous as to disprove the rule. Ideally, the study of landlordism in Ireland, as elsewhere, requires an aggregate approach. Case histories of great estates—usually so much better documented than small estates—may reveal many important facets of the landlord system; but, without knowledge of how medium- and small-sized estates functioned in the same period, the extent to which the great estate was typical of the whole can never be fully understood.⁶ During the 1870s

⁴ For works by these scholars, which epitomize concern for evidence and objectivity; see Maguire, *The Downshire Estates in Ireland, 1801–1845* (Oxford, 1972); Solow, *The Land Question and the Irish Economy, 1870–1903* (Cambridge, Mass., 1971); Donnelly, *The Land and the People of Nineteenth-Century Cork* (London, 1975); and Vaughan, “Landlord and Tenant Relations in Ireland, 1850–1878” (Ph.D. thesis, University of Dublin, 1974).

⁵ It is easy to manipulate the rather ambiguous statistics relating to eviction and to assume that all ejectment proceedings ended in final (not to say, forcible) expulsion from holdings. Contrary to partisan accounts, very few landlords relished evictions, and they usually had a hard time finding tenants prepared to take on the stigma and the strain of working an evicted holding. Eviction did not always mean final separation from a holding; according to one official estimate, 21,340 of the 90,107 families evicted from their farms between 1849 and 1880 (23.7 percent) were readmitted as either caretakers or tenants. Solow has contended that the percentage of evicted families readmitted in the early 1880s ranged between 40 and 50 percent, and she has estimated that the overall eviction rate between 1855 and 1880 was less than 3 percent of the six hundred thousand holdings in 1855. Solow, *The Land Question and the Irish Economy*, 54–57. There were, of course, some harsh and large-scale clearances on various estates during and after the Famine, and no amount of statistical finesse can prove that the suffering caused by these evictions was a figment of the Land League’s imagination. Fear of eviction was widespread among the poorer tenantry, and the “collective folk memory” of eviction formed a vital part of both Irish folklore and agrarian political consciousness. Also see F. S. L. Lyons, *Ireland since the Famine* (London, 1971), 103–04, 156, 159–60. And, for allusions to the callous behavior of some landlords during the Famine, see Cecil Woodham-Smith, *The Great Hunger* (London, 1962), 70–72, 123–24, 163–64, 183, 298–99, 324–28.

⁶ See, for example, Maguire, *The Downshire Estates in Ireland, 1801–1845*. Probably no other resident Irish

Ireland had more than six thousand "substantial" estates—defined as holdings in excess of five hundred acres or £500 annual valuation.⁷ Given the number of and profound disparities between estates, it is risky to draw even mildly dogmatic conclusions about Irish landlords as a whole from the anatomy of one or two estates, no matter what their size and resources.

The powerful forces of neglect and destruction have been at work on Irish records long enough to rule out the chance of obtaining a full accounting of landed wealth and indebtedness in Ireland. Only in rare instances do the surviving documents contain a reasonably complete and reliable record of an estate's financial condition, and even then the rent books and accounts do not shed much light on the social relations prevailing on that particular estate over time.⁸ For a class commonly reputed to have possessed as many debits as credits, little is known about the nature and extent of landlord indebtedness during the nineteenth century. Like the proverbial iceberg, only what lies above the water line is visible, even if a decent run of rentals and accounts is available. But measuring with accuracy the bulk of the ice—or indebtedness—lying beneath the surface is no simple matter. What follows is an attempt to explore the dimensions of landed (or landlord) indebtedness in post-Famine Ireland. Beyond this objective lie the possibilities, however tenuous, of advancing some arguments about the consequences of indebtedness on certain kinds of estates and making some connections between the economic problems of the landlords and their responses to the social and political unrest of the Parnellite era.⁹

Several assumptions underlie this study, and these should be exposed before proceeding to examine the material evidence. The first assumption involves the existence of a vital and close connection between a landlord's debts and assets—that is, between his annual income and the annual operating costs arising out of his estate and his family. In short, a landlord's net worth, once he had deducted his fixed costs and overhead, weighs far more heavily in this study than does his gross rental—that is, the sum of the rents and arrears due to him twice a year. The incumbrances or capital debts of Irish landlords may help show something about the actual solvency, as distinct from the reputed wealth, of estates great and small. And solvency is the product of a constant interplay between annual rental income and the myriad expenses of running an estate and maintaining the life-style appropriate to a landed gentleman.¹⁰

landowner possessed the wealth, resources, and credit of the Marquesses of Downshire. Their lands in six counties had an official valuation of £91,563 and amounted to 110,273 statute acres.

⁷ These are the criteria used by U. H. Hussey de Burgh in his useful but erratic compilation, *The Landowners of Ireland* (Dublin, 1878), based on the government's "Return of Owners of Land in Ireland," *Parliamentary Papers* [hereafter, *PP*], 1876, 80: 422.

⁸ Both public and private archives in Ireland contain many fragments of estate papers, including runs of rentals and accounts and "out-letter books," which consist of copies of letters written by the agent. But there are relatively few complete collections of family and estate records. In some cases landed families entrusted the bulk of their deeds and financial documents to the care of solicitors who kept or destroyed all of the papers in question. Documents pertaining to the most heavily incumbered estates were usually the first to disappear, and, since much more documentation has survived about solvent estates, there is a danger that the evidence creates a false impression of more affluence than insolvency.

⁹ As used here, the term "landlord" refers to owners of tenanted land. Almost all substantial owners were landlords. But there were just enough small landowners in Ireland who had no formal tenancies to justify preserving the slender distinction between these two terms.

¹⁰ Landlords receiving an annual remittance or "profit" from their estates of at least 30 percent of the rents

The second assumption of this study is the presence of a widespread credit-debit nexus in Irish rural society. Both landlords and tenants, in other words, depended for their well-being, if not for their survival, upon an elaborate and secretive system of short- and long-term loans. In the post-Famine period, after the rural economy had become fully monetized, virtually every tenant farmer owed money to someone at one time or another. If the tenant could not afford the interest rates of the local moneylender or gombeen man, he might find a friend or relative who would lend the necessary amount—to be repaid, he hoped, once the pig, the bullock, or the harvest had been sold.¹¹ A crop failure or sharp fall in prices could, of course, wreak havoc on this vulnerable credit system. In times of prolonged distress, notably after 1878, both the borrowers and the moneylenders suffered severe losses. What is too often forgotten is that landlords were just as dependent on credit as were their tenants—only on a much grander scale. Anything likely to cause hardship among the tenantry was bound, sooner or later, to affect the economic position of the landlords. Mounting arrears of rent could hardly be used to pay the coach maker, the wine merchant, or the tailor, not to mention the unrelenting moneylender in Dublin or London. However different the motives and mechanisms of landlord and tenant indebtedness and however higher the credit rating of the man of property, the closeness of the landlord-tenant connection made it virtually impossible for either party to escape unscathed from the misfortunes of the other. Any downturns in the agricultural economy, for example, could not help but embarrass those landlords and tenants who were already paying more in the way of interest than they could afford in the best of years.

ACCORDING TO THE NOVELS OF ANGLO-IRISH GENTRY LIFE, all landed families were seemingly engulfed in debt and all owners were apparently plagued by parasitical moneylenders looking for a chance to profit from the misfortunes of an ancient family. From *Castle Rackrent* to *The Big House of Inver*, the novelists of landed society depicted a class that habitually lived beyond its means.¹² Because credit came so easily to landowners many of the Irish gentry mortgaged their estates to the hilt in order to impress others with their ability to afford not only the necessities but the luxuries of the social seasons. The obsession of many pre-Famine Irish landowners with conspicuous spending meant that the stock figure of the fortune-hunting squire and squireen was not just a figment of the novelist's imagination. The younger sons, brothers, and nephews of the "mere gentry" produced more than their share of rakes and adventurers whose heavy gambling, drinking, wenching, and duelling enlivened not only the novels of Edgeworth, Lever, Lover, and Thackeray but also Irish life.

received, after the agent had paid all the bills and costs of incumbrances, should be considered solvent. Any amount less than this would have made it difficult for a gentleman dependent on rental income to keep himself and his family in the manner appropriate to his social standing.

¹¹ For insights into the operations of this rural credit system, see the "Report of the Departmental Committee on Agricultural Credit in Ireland," *PP*, 1914-16, 80, Cd. 7675: 51-62.

¹² Both Maria Edgeworth and Edith Somerville, along with "Martin Ross," had direct experience of dealing with tenants and managing estate finances.

If nineteenth-century novelists treated the foibles of Irish landed society with both irony and satire, the realities of the Big House were not far removed from the world of the Rackrents, the Clonbronzys, and Barry Lyndon. The agricultural boom of the late eighteenth and early nineteenth centuries increased the appetite of many landowners for the symbols and comforts of wealth. As in England, so in Ireland the mortgage loan was the indispensable instrument of social as well as economic progress. Few landowners managed to avoid borrowing money at some stage in their lives.¹³ By means of loans scattered among various creditors, some of whom were relatives and others professional moneylenders, owners could continue to maintain a privileged lifestyle long after their rental income had ceased to pay the bigger bills. When crop failures or depressions occurred, landowners who had mortgaged their properties to the legal limit found it well-nigh impossible to pay their interest installments due twice a year. By the unwritten law of the survival of the least incumbered estates, owners who had exhausted their credit with the moneylenders and who had found no heiresses to wed were the first to experience the indignity of receivers appointed over their estates or, after 1849, to witness the sale of their lands in the Incumbered Estates Court.

Conspicuous consumption might not have caused Irish landowners quite so much grief had they enjoyed the additional sources of income available to so many of their English and Scottish counterparts.¹⁴ Ownership of coal mines, canals, and property ripe for urban development or investments in commercial and industrial enterprises might have helped to pay for the aristocratic tastes of the Irish gentry. But no more than 1 percent of all resident Irish landowners was fortunate enough to own either profitable coal mines, urban estates in Dublin or Belfast, or good farm land in England. When an estate had been mortgaged to the legal limit, the owner, if desperate for ready cash, might struggle to raise a second or even a third mortgage with some other—presumably unsuspecting—creditor. But it was only a matter of time before such estates were declared insolvent and put up for sale in the courts created to dispose of them.¹⁵

Like poverty, wealth is relative, and many Irish landowners lived in a financial limbo, caught somewhere between luxury and austerity. The degree of comfort was determined by the quality of the harvest, price fluctuations, and the presence of a dowager whose maintenance—by means of a jointure—could drain a small estate over the years. Of the various causes of landed indebtedness

¹³ For examples of heavy mortgages and family charges, see Maguire, *The Downshire Estates in Ireland, 1801–1845*, 65–106; and R. F. Foster, *Charles Stewart Pamell* (London, 1976), 49–53, 98–104, 127–30, 190–96. For mortgaging activity by English landowners, see David Spring, *The English Landed Estate in the Nineteenth Century* (Baltimore, 1963), 35–40, 189–90, and “The English Landed Estate in the Age of Coal and Iron, 1830–80,” *Journal of Economic History*, 11 (1951). Also see the writings of F. M. L. Thompson on English landownership; for convenient citations to them, see David Cannadine, “Aristocratic Indebtedness in the Nineteenth Century: The Case Re-opened,” *Economic History Review*, 2d ser., 30 (1977): 625 nn. 1–4. Cannadine has also referred to landowners who borrowed from institutions, friends, and relations; *ibid.*, 633–37.

¹⁴ For the business interests of English landowners, see F. M. L. Thompson, *English Landed Society in the Nineteenth Century* (London, 1963), esp. 256–68, 317–26. Also see the articles by David Spring cited in his *The English Landed Estate*, 201; J. T. Ward and R. G. Wilson, eds., *Land and Industry: The Landed Estate and the Industrial Revolution* (Newton Abbot, 1971); and Richard Perren, “The Landlord and Agricultural Transformation, 1870–1900,” *Agricultural History Review*, 18 (1970): 36–51.

¹⁵ See the returns of sales of insolvent estates in the Landed Estates Court and its successor, the Land Judges Court, *PP*, 1880, 60, no. 377, and *PP*, 1890, 60, no. 74.

by far the most tangible was the construction of a country house, a castellated or palladian showplace surrounded by elaborate pleasure grounds. But so obvious an explanation can mislead, and there is scant evidence to prove that owners of Irish estates slid into bankruptcy under the pressure of building mortgages alone. The eagerness of Irish landowners in the eighteenth and early nineteenth centuries to emulate the English aristocracy did indeed drive them to spend beyond their immediate means on building an impressive Big House;¹⁶ but the building boom of the late Hanoverian era derived much of its impetus from the prosperity of agriculture in a time of increasing demand for foodstuffs in the British Isles. After the 1760s, rents began to rise in response to the pressure of an expanding population on both land and farm produce, and the increased value of land made moneylenders all the more willing to do business with the gentry and aristocracy.¹⁷

Most Irish landowners who built new houses after the 1760s seem to have taken the costs in stride. The exceptions included small landowners whose appetites for display did exceed their ability to pay and also compulsive spenders like the famous Bishop of Derry (Frederick Hervey, Earl of Bristol), who commissioned two palatial houses in county Londonderry during the 1770s and 1780s. Hervey had inherited a magnificent house and estate in Norfolk, but his income did not permit him to maintain three great houses—and he never spent a night in Ballyscullion, which had cost almost £80,000. His passion for building and living on a grand scale engulfed him in debt and certainly hastened his financial collapse.¹⁸ Of other cases of costly Big Houses, few came close to Hervey's extravagance. Castle Coole, near Enniskillen in county Fermanagh, cost the first Earl of Belmore some £54,000 during the 1790s; and the second Earl of Gosford paid almost £80,000 for his crenellated showplace, Castle Gosford, in the 1830s.¹⁹ But in neither case could the owner's financial difficulties be traced directly or exclusively to building expenses.

A number of other costs, especially those arising out of family charges, accounted for the heavy incumbrances that eventually forced landowners to part with their properties by means of voluntary or involuntary sale. In the long run, the cumulative cost of an aristocratic life-style drove many owners of estates to the moneylender. No single expense but the combination of all of the bills associated with running an estate and maintaining high status tended to exhaust an owner's income. When a town house in London became a necessity for most

¹⁶ Too many of the recent books on Irish country houses belong on the coffee table rather than on a desk. There is need of the kind of historical analysis found in Mark Girouard's *Victorian Country House* (Oxford, 1971) and Lawrence and Jeanne Stone's "Country Houses and their Owners in Hertfordshire, 1540–1879" in W. O. Aydelotte et al., eds., *The Dimensions of Quantitative Research in History* (Princeton, 1972), esp. 115–23.

¹⁷ For a discussion of the agricultural and commercial development of the Irish economy after 1750, see L. M. Cullen, *An Economic History of Ireland since 1660* (London, 1972), 77–103; and Geraróid Ó Tuathaigh, *Ireland before the Famine, 1798–1848* (Dublin, 1972).

¹⁸ For the Earl-Bishop of Derry's building mania, see Constantia Maxwell, *Country and Town in Ireland under the Georges* (Dundalk, 1949), 88–90; and, for an allusion to his other passions, see Brian de Breffny and Rosemary Holliott, *The Houses of Ireland* (London, 1975), 166–68.

¹⁹ For the building of Castle Coole, see Maxwell, *Country and Town in Ireland under the Georges*, 90–91; and Desmond Guinness and William Ryan, *Irish Houses and Castles* (Dublin, 1971), 163–68. For Castle Gosford, see William Greig, *General Report on the Gosford Estates in County Armagh*, ed. F. M. L. Thompson and D. Tierney (Belfast, 1976), 8–9. In 1817 Lord Gosford was spending roughly 57 percent of his rental income on interest arising out of mortgages and on annuities.

Irish peers and the wealthiest gentry after the passing of the Act of Union, a host of new expenses as well as new temptations came into play and often stretched an estate's resources to the breaking point.

With the exception of losses at the gaming table, some of which had devastating effects on the gambler and his family, expenditure on what has been called "the paraphernalia of gentility" had only a nuisance value when compared with the cost of family charges incurred through marriage settlements. In his pioneering study of Irish landed wealth in the latter eighteenth century, David Large discovered that the actual operating costs of an estate, including money spent on improvements, accounted for only a small fraction (in some cases less than 15 percent) of the annual rental. Big House construction and a lavish social life could, indeed, deplete an estate's resources, but even the extravagant second Baron Cloncurry managed to survive the heavy costs of building Lyons in county Kildare. Without benefit of quantification Large attributed most of the indebtedness on the estates in his small sample to charges arising out of family settlements. As Edward Wakefield observed long ago, even great landowners knew the anguish of watching "the whole of their fortune being absorbed either by payment of a mother's jointure, the fortunes bequeathed to brothers or sisters, or debts contracted by themselves or left them by their predecessors." And, when dowagers survived their husbands by an average of twenty-one years, as did the widows of Irish peers in the 1780s, the expense of jointures appropriate to a countess or marchioness could not be taken too lightly.²⁰

If marriage settlements devoured landlord incomes in the late eighteenth century, as Large has contended, it is not hard to imagine how much heavier the burden of three or four generations of such charges could be, especially when the rental income on many estates had barely doubled between 1815 and 1875. Only a rare combination of "fortuitous" circumstances—the early demise of a dowager, an heir apparent who did not gamble, and a marriage alliance with an heiress bearing a handsome dowry—could have relieved lesser landowners of that cumulative debt burden that drove so many of that class to the brink of insolvency—and beyond—after 1878.

Although far too fragmentary, the evidence for the post-Famine period does indicate that family charges, especially portions for younger children, accounted for the heaviest incumbrances on many Irish estates. Providing capital sums for the children of landed families tied up precious assets at a time when these might have been gainfully invested in commercial or industrial enterprises yielding 4 or 5 percent. The dictates of custom and status forced landowners to incumber their estates with huge liabilities in order to keep their closest kin in comfort; and such obligations could easily cripple an estate over several generations, especially if the portions were generous and large arrears of rent were tolerated.²¹ Had landowners kept marriage settlements down to a minimum of

²⁰ Large, "The Wealth of the Greater Irish Landowners, 1750-1815," *Irish Historical Studies*, 15 (1966): 33-35, 37, 38.

²¹ Although there are no comparative studies of the debt burdens on English, Scottish, Irish, and Welsh estates, it is reasonable to assume that English (and a few Scottish) aristocrats had an easier time coping with their incumbrances than did all but a handful of Irish or Anglo-Irish peers who owned property in Great Britain. For present purposes and in spite of the ambiguities of "ethnic origins," such peers as the Earl of Dunra-

support and had they invested more capital in stocks and bonds, they would have had far less need of the services of professional moneylenders. Portions of up to £1,000 for each younger child were common enough among the substantial gentry, and peers above the rank of viscount often made provision for sums up to £10,000 per child when the estate was settled or resettled at the time of the heir apparent's marriage. In 1856 some 40 percent of the incumbrances on the Downshire estates derived from family charges or marriage portions, the result of "successive family settlements since 1741." Such charges might not have embarrassed owners while harvests were good and prices high, but every decade had its poor seasons and the Great Famine of 1845–48 left landowners ruining the day when they or their ancestors had tied up so much capital in wholly unproductive investments. When agriculture fell upon hard times in the early 1860s and again, more dramatically, in the late 1870s, incumbered owners had to turn and return to the moneylender for transfusions of cash sufficient to maintain appearances or pay the outstanding interest on previous loans.²² Obsession with status and commitment to familial privilege thus burdened owners with debts that passed from owner to heir until the day of reckoning.

As the scion of an old Irish landed family has observed, many landowners simply "could not resist the temptations of prosperity."²³ Even the richest landowners found it hard to avoid the temptations of low interest loans. Maguire's meticulous study of the Downshire estates reveals just how easily credit came to a magnate of the marquess's stature. His land agents actually had to rebuff offers of financial help from merchants, lawyers, and neighboring landowners with capital to spare, who were looking for a "blue chip" investment.²⁴ To be the creditor of a landed magnate must have brought social as well as financial profit to an ambitious and unaristocratic investor. Such eagerness to underwrite the obligations of a great landowner for a mere 4 to 4.5 percent interest rate could only have whetted the appetite of those with a taste for luxury goods and services. Not all Irish estates, of course, looked as attractive to mortgagees as did Lord Downshire's, for here was an owner who could and did borrow some £186,500 between 1810 and 1840 without straining his resources.

In general, then, the larger or wealthier the estate, the easier loans at low interest were to obtain. On small estates, where owners depended wholly on rental income, a large loan might prove harder to find, depending on the state of the money market and the charges already weighing upon the owner or tenant-for-life. Heavily incumbered owners might have to pay as much as 5 to 5.5 percent interest in the 1870s to compensate for the greater risk to the creditor. Peers, on the other hand, usually enjoyed interest rates around 4 percent, and the only commoners who could match their high credit rating were the rare owners of lucrative coal mines, like the Prior Wandesforde's of Castlecomer, county Kil-

ven, the Marquess of Londonderry, and the Earl of Meath are considered here as Anglo-Irish, whereas the Duke of Devonshire, the Marquess of Lansdowne, the Earl of Pembroke, Baron Leconfield, and Lord John Russell, all of whom owned large estates in Ireland, are classified as being more English than Irish.

²² See James S. Donnelly, "The Irish Agricultural Depression of 1859–1864," *Irish Economic and Social History*, 3 (1976): 33–54; and Solow, *The Land Question and the Irish Economy*, 168–94.

²³ Conversation with the Hon. Bryan Bellew, March 2, 1970, Barmeath Castle, Dunleer, county Louth.

²⁴ Maguire, *The Downshire Estates in Ireland, 1801–1845*, 101–04.

kenny, or a great linen manufacturer, like John Mulholland of Belfast, who became Baron Dunleath in 1892. In virtually every mortgage, what mattered most was not the face amount or principal but the relationship of the interest due on that sum to actual income.²⁵ The dimensions of this "debt burden," the percentage of rental income paid out to service these loans, as well as its implications constitute the principal concern of this study.

Two basic questions emerge: first, roughly how much of the annual income of these estates went to pay interest on mortgages and other family charges, and, second, what could incumbered owners afford to spend on servicing those debts before they began to sink into insolvency? Given the disparity of resources and management practices on Irish estates, these questions admit of no easy or precise answers. Deficiencies of evidence, moreover, are an obstacle to understanding: countless estate papers have been lost or destroyed, and the records of family solicitors and private moneylenders have not surfaced. To make matters worse, the sample of incumbered landlords used in this study is too small—two hundred and twenty-five, or only 3.7 percent of the substantial landowners of Ireland—to constitute the degree of proof one would prefer. When faced with such obstacles, historians are wont to say, rather defensively, that a small sample is better than none, and that is the case here.

Most of the documentation comes from two sources, the Church of Ireland and the Irish Land Commission.²⁶ The first set of records consists of the mortgage ledgers of the Representative Body of the Church of Ireland (or RCB). Under the Disestablishing Act of 1869 the Church of Ireland received compensation of almost £8.5 million, and the annual yield from this endowment helped defray operating expenses, including subsidies to needy clergymen.²⁷

²⁵ Interest rates varied in accordance with money supply, credit rating, connection, timing, and other factors, but they remained remarkably stable for mortgages from the 1850s to the early 1880s. In the 1830s a reasonably wealthy landowner could usually obtain a mortgage loan at 3.75 percent and in the early 1870s at 4 to 4.25 percent. Small landowners with less "margin" often had to pay close to 5 percent. The penalty for borrowers who fell behind in their payments could run up to 1 to 1.5 percent over the standard rate. See page 341, note 28, below. Even heavily incumbered landlords could borrow more money by the simple expedient (allowed under Irish law) of declaring themselves by deed to be judgment debtors, which entitled them to raise loans on the security of their land. See W. L. Burn, "Free Trade in Land: An Aspect of the Irish Question," *Transactions of the Royal Historical Society*, 4th ser., 31 (1949): 69. "Indebtedness" primarily means those mortgages and other debts, including borrowings by bond or personal arrangement, on which interest had to be paid. This category does not, therefore, include any government loans authorized in the various land improvement and drainage acts passed after 1847, which enabled the Board of Works to advance roughly £3.1 million at 3.5 percent to landowners who wished to drain, fence, or reclaim their land. See Cormac O Gráda, "The Investment Behaviour of Irish Landlords, 1850–75: Some Preliminary Findings," *Agricultural History Review*, 23 (1975): 139–55. Because these loans involved a modest outlay for the borrower and were voluntary, I treat them here as part of the annual operating cost of an estate. Indeed, such an investment could lead to a gradual increase in both productivity and rental, sufficient in some cases to ease the owner's debt burden by a modest amount. Where the evidence permits, such lesser liabilities as personal debts by bond or private agreement and bank overdrafts have been counted as part of the overall debt burden. Unfortunately, few estate papers contain records of an owner's personal finances, except to register the remittances or profits from the rents received that were deposited in his private bank account.

²⁶ I am most grateful to the secretary and staff of the Representative Body of the Church of Ireland, Church House, Rathmines, Dublin, for permission to work on the mortgage ledgers and other records in their keeping. Thanks are also due to the secretary of the Irish Land Commission (at the time of my research, T. O'Brien) for allowing me to examine the records of the Estates Commissioners pertaining to sales under the Wyndham Act, and also to the trustees of St. Patrick's College, Maynooth, for permission to see the papers relating to the college's seven mortgage loans dating from the 1870s.

²⁷ For the financial provisions of the Irish Church Act of 1869, see Hugh Shearman, "Irish Church Finances after the Disestablishment," in H. A. Cronne *et al.*, *Essays in British and Irish History* (London, 1949), 278–302.

In 1870, the RCB set up a Finance Committee to manage or invest the capital in safe securities. Because land was still considered one of the soundest possible investments in the early 1870s, the committee sank almost half of the RCB's capital in mortgages on Irish estates at interest rates ranging from 4 to 4.5 per cent.

In theory the Finance Committee approved loans only to those applicants whose estates had been found solvent after careful inspection of the books by its solicitor. In practice, however, connection or influence may have helped the owners of some heavily incumbered estates to obtain mortgages from the RCB. Since most of the mortgagors were either parochial or diocesan pillars of the Church, it is reasonable to assume that the Church used its capital not only to realize profit but also to support loyal members of the Church (and the Ascendancy) who needed financial help. Once the word spread that the RCB was prepared to extend loans secured by land at low rates, applications poured into Church House. Most of the requests came from owners who wanted to consolidate their numerous debts into one giant mortgage loan, which would greatly simplify their payment problems while it ensured a low interest rate. No doubt owners also looked forward to a long and happy association with a mortgagee whose spiritual credit and goodwill could not be impugned.

Between 1871 and 1876 the Finance Committee approved one hundred and twenty requests for loans and may well have turned down four to five times that many applications. In these six years the committee accepted 82.8 percent of the total of one hundred and forty-five mortgagors whose names appear in the ledgers up to 1920.²⁸ At its peak in 1877–78 the RCB's mortgage fund amounted to £3.5 million, all of which was tied up in Irish estates. The onset of agricultural depression and agrarian unrest in the late 1870s alarmed all investors in Irish land, and the Finance Committee (or FC) called a halt to all mortgaging operations for several years. Only ten new mortgages were approved between 1877 and 1895.²⁹ The balance of the RCB's capital was placed in such "safe" securities as railway shares and debentures as well as Russian and Ottoman imperial bonds. (These imperial bonds, needless to say, became less than safe after 1914.) Had it not been for the various land purchase acts (1885–1909), which subsidized the sale of tenanted land to the occupiers, few of these mortgagors would

Also see Donald Akenson, *The Church of Ireland* (New Haven, 1971), 260–68, 309–20; and Michael Hurley, ed., *Irish Anglicanism, 1869–1969* (Dublin, 1970), 14, 18, 21–22.

²⁸ The average interest rate for the one hundred and twenty mortgage loans in this sample was 4.32 percent. Only a few of the richest borrowers managed to secure a rate of 3.75 percent from the RCB, whereas most of the smaller landowners had to pay 4.5 percent with a penalty rate of 5 to 5.5 percent. As for adequate security, the FC tried to follow the policy that "the sum lent should never exceed what would leave a surplus of at least half the rental after payment of the mortgage interest." RCB, *31st Report of Proceedings . . . 1901*, Church House, Rathmines. In other words, owners whose debt burden would be in excess of 50 percent counting the mortgage requested from the RCB were, at least in theory, not eligible for a mortgage loan.

²⁹ Between 1871 and 1920 the RCB approved a total of one hundred and forty-five loans to one hundred and forty individuals, taking into account different loans to the same owner over time. (There were two transfers of mortgages from the original mortgagor to his heir.) Four mortgagors could not be located in the "Return of Owners of Land in Ireland, 1876," and these have been omitted from the sample here. The chronological distribution of these loans is as follows: 1871–76, 120 loans approved; 1877–79, 0; 1880–85, 10; 1886–95, 0; 1896–99, 3; 1900–11, 5; 1912–20, 7. Mortgage Ledgers D1–D3 and Y, Church House, Rathmines [hereafter, ML/RCB]. Also see the *Annual Reports* of the RCB from 1871 to 1901. The amount invested by the RCB in mortgages reached an all-time high of £3,512,598.8 in 1876. Interest on this sum amounted to £154,825.8 in 1877 (equivalent to an average interest rate of 4.4 percent).

have been able to repay principal to the RCB, which was severely buffeted by the death or exodus of so many communicants during and after the Great War.

The second major set of records derives from the Irish Land Commission, which originated the Final Schedule of Incumbrances (or FSI). When landlords came to sell portions of their estates under the land purchase acts, their solicitors had to submit a host of documents to the Land Commission giving particulars as to location, size, rental, valuation, and charges pertaining to the lands in question. Under the laws of real property, all who had a superior interest in the land being sold were entitled to compensation out of the proceeds of the sale. In their applications to the commission, the landlords' solicitors itemized all of the charges on the lands being sold along with the names of the incumbrancers. The FSIs thus contain summaries of every charge, ranging from head rents, quit rents, and tithe-rent charges to mortgages and solicitors' fees for preparing the necessary information.³⁰ Running at times to twenty to thirty pages, the FSIs provide some fascinating, if fleeting, insights into the shadowy world of money-lenders, where mortgages and judgments for debt were often assigned and re-assigned from one creditor to another over several generations. Because most of the FSIs contain the land court judges' rulings on the validity of each claim, it is possible to calculate the amount of money paid out of the original purchase money to the various claimants as well as the balance or residue left to the actual vendor.

Neither the FSIs nor the ecclesiastical mortgage ledgers tell the whole story of landed indebtedness in Ireland, but they do constitute two of the largest pieces of a puzzle from which so many other pieces are missing, and they reveal enough about the extent of incumbrances to permit some assertions about the nature and extent of landed indebtedness in the post-Famine period.

THE AVAILABLE MORTGAGE DATA begin to make sense only when linked to income. Without some idea of the actual earnings of these incumbered estates, the assorted mortgage loans are about as useful as an inventory or laundry list. The relative scarcity of rent books and accounts, however, means that income in the majority of cases has to be estimated or improvised, and this calculation is always risky. The most useful source from which to derive such an improvisation is the official valuation carried out by Sir Richard Griffith's staff between 1852 and 1865.³¹ As every Irish historian knows, Griffith's valuation was never in-

³⁰ For instructions concerning the drafting of the Final Schedule of Incumbrances under the various land purchase acts, see George Fottrell and John G. Fottrell, *Practical Guide to the Land Purchase Acts (Ireland)* (Dublin, 1891), 306–09, 476–80, 517, 652–55. Also see R. R. Cherry, *The Irish Land Law and Land Purchase Acts, 1860 to 1891* (Dublin, 1893), 607–09, 644, 659. FSIs deriving from sales prior to 1903 were not available for inspection in the Records Branch of the Land Commission. For a brief introduction to these documents, see K. L. Buckley, "The Irish Land Commission as a Source of Historical Evidence," *Irish Historical Studies*, 8 (1952): 28–36.

³¹ For details of Griffith's valuation, see Solow, *The Land Question and the Irish Economy*, 59–68; Select Committee [House of Commons] on General Valuation, Etc. (Ireland), *PP, 1868–69*, 9, no. 362; and the Irish Land Committee's pamphlet, "The Land Question, Ireland, No. 1: Notes upon the Government's Valuation of Land in Ireland" (June, 1880). For our purposes, the valuation is least useful, *mutatis mutandis*, as a guide to net rental in those counties surveyed during the early 1850s—most of which lay in Leinster and Munster. The valuers assessed Ulster and parts of Connaught in the latter 1850s and early 1860s, when prices had recovered from the depression of the Famine years.

tended to serve as a measure of rental or sale value; rather, it was designed to provide a basis for local taxation. The valuers based their assessments on the prevailing prices of such staples as wheat, oats, barley, butter, and livestock, and they also took into account access to urban markets for those goods. Since the prices for all of these staples fluctuated sharply between the 1850s and 1880s, the valuation could not be expected to reflect the hypothetical earning power of holdings or estates much before or after the valuers' visit. If, however, there are some compelling reasons for not treating Griffith's valuation as equivalent to rental income, there are even stronger reasons for doing just that, because, ironically enough, the valuation does provide a rough guide to rents actually received on many estates after 1878.

As poor harvests and falling prices eroded farmers' income after 1878 and as tenants in increasing numbers began to obey the Irish Land League's orders to pay their landlords no more than the equivalent of Griffith's valuation, arrears of rent soared upward and rental income fell correspondingly. Analysis of the rents received on two dozen estates (mostly from the years 1875 through 1884) reveals a close proximity between valuation and income. The average rents received on these twenty-four estates over a ten-year period exceeded the valuation by only 1.7 percent.³² Admittedly, most of these estates lay in eastern Ulster and Leinster, where rents tended to be paid more regularly than in the west, especially during the land war. It is important to note the difference between the rental (rents due) and rents received: on twenty-three of these same estates, annual rents due exceeded not only rents received by 2.7 percent but also the valuation by 4.1 percent. In other words, the rents collected on these estates came much closer to the valuation than did the nominal rental.

Adverse economic conditions in the latter 1870s, which were not confined to Ireland or even the British Isles, and agrarian agitation organized by the Irish Land League combined to depress rental incomes to the point where many owners were receiving the rough equivalent of Griffith's valuation, no matter how arbitrary or outdated that valuation may have been. Another cause of the shrinking incomes of landlords after 1881 was the operation of the fair rent clauses of Gladstone's second Land Act. Between 1882 and 1893 the rents on 288,054 holdings all over the country were reduced by an average of 20.7 percent.³³ For these compelling reasons, then, the rental income on virtually all Irish estates began to fall off significantly from the peak attained between 1875 and 1877.

³² Of these twenty-four estates, nine were in Ulster, eight in Leinster, five in Connaught, and two in Munster. Average rents received on these estates over a decade came to £283,487.6, and the total valuation was £278,785—a difference of 1.7 percent. Where possible the rentals in this sample covered both the peak years of income in the mid-1870s and the trough years of the early 1880s. For permission to use these rentals and accounts, I am grateful to the owners or trustees of the following estate papers, from the Public Record Office of Northern Ireland [hereafter, PRONI]: Abercorn, Antrim, Downshire, Erne, Gosford, Ranfurly, and Stewart; and from the National Library of Ireland [hereafter, NLI]: Clonbrock, Crofton, Drake, Hodson, Inchiquin, Leitrim, Leslie, O'Hara, and Ormonde. For the use of rentals and accounts in private hands (at the time they were examined), special thanks are due to Col. Errold Cosby of Stradbally, the Earl and Countess of Dunraven, the trustees of the Pembroke Estate, Josephine O'Connor of Clonalis, the Hon. Thomas Pakenham of Tullynally Castle, and the late Lord Talbot de Malahide of Malahide Castle.

³³ "Report of the Irish Land Commissioners . . . 22 August 1891 to 31 March 1893," *PP*, 1893-94, 24, C. 7056, Tables 4, 48.

TABLE 1
Mortgage Loans of the Representative Church Body, 1871-1907

<i>Estate Valuation in £</i>	<i>Number of Estates</i>	<i>Total Valuation in £</i>	<i>Total Mortgage Loans</i>	<i>Average Mortgage Loan</i>	<i>Ratio: Mortgage/ Valuation</i>	<i>Annual Interest Due</i>	<i>Average Rate of Interest</i>	<i>Debt Burden %</i>
SMALL: 100-1,000	34	18,938.8	299,176.5	8,799.3	15.8	13,420.0	4.49	70.9
MEDIUM: 1,000-5,000	63	147,998.7	1,350,050.4	21,429.4	9.1	60,112.5	4.45	40.6
LARGE: 5,000-15,000	11	111,566.0	678,198.5	61,654.4	6.1	28,688.2	4.23	25.7
GREAT: Over 15,000	12	321,584.5	1,484,365.0	123,697.1	4.6	62,390.5	4.20	19.4
TOTALS: All Estates	120	600,088.0	3,811,790.4	31,764.9	6.4	164,611.2	4.32	27.4

SOURCE: Mortgage Ledgers, D1-D3, Y, RCB MSS, Church House, Rathmines, Dublin.

Further evidence of the correspondence between rents and valuation can be found in the reports of the Irish Land Commission concerning sales of estates under the Ashbourne Act of 1885. According to these summary returns, the tenement valuation of all of the holdings sold under that measure up through March 31, 1890, came to £274,358.8, yet the gross rental on those properties was £300,370.7.³⁴ Thus rents due exceeded valuation by 8.7 percent. But this figure must be adjusted downward by at least 5 percent in order to cover the gap between rents due and rents received, a gap of particular importance for the years after 1885, when both rent strikes and the fixing of fair rents caused serious erosions of income on many estates.

In sum, the rents received on many Irish estates after 1878 came much closer to Griffith's valuation than has been hitherto realized. And the convergence of rental income over a three- or five-year span with the official valuation of those estates makes it possible to use the valuation as a rough and ready guide to the income on those estates for which mortgage data are available. The imperfections of Griffith's valuation and the small size of the sample used here are bound to invite some skepticism. But only by taking such risks can we move beyond the elementary stage of enumerating the incumbrances of a few Irish landlords in the late nineteenth century. Using the valuation of those estates whose owners managed to obtain loans from the RCB, an estimate of the percentage of rental income spent on interest during the last quarter of the nineteenth century is possible. Table 1 shows the estimated debt burdens of the one hundred and twenty mortgagors who borrowed over £3.8 million from the RCB between 1871 and 1907.³⁵ A standard interest rate of 4.5 percent has been applied across the board.

Breaking the RCB data down into categories of wealth or income leads to some rather interesting results. Distributing the estates of the RCB mortgagors within a schema of small, medium, large, and great estates, again based on valuation, demonstrates the contrasts between the debt burdens of relatively rich and poor landowners.³⁶ The average debt burden of these one hundred and twenty mortgagors, using valuation as roughly equivalent to income, comes to 27.4 percent. Owners of small estates in this sample had to pay slightly over 70 percent of their estimated income to the RCB, whereas the owners of great estates were paying just under 20 percent in interest.³⁷ These estimates point to a conclusion that should cause no surprise among students of income distribution in capitalist societies: the absolutely rich could afford to borrow huge sums, while the relatively poor had a hard time coping with even modest loans. The

³⁴ "Return of Landowners . . . Purchase of Whose Properties . . . Sanctioned by the Irish Land Commission," *PP*, 1889, 61, no. 81: 1-17, and *PP*, 1890, 60, no. 115: 1-12.

³⁵ In view of the 1.7 percent discrepancy between rents received and the valuation found in the sample of twenty-four estates above, some readers may wish to add 1.7 percent to the valuation of the one hundred and eleven estates in Table 1—making a total of £598,816.6—in order to arrive at a more "realistic" estimate of rental income. Using this figure, the total debt burden falls only half a percentage point—down to 27 percent. The data in Table 1 derive from ML/RCB, D1-D3, Y.

³⁶ However crude these categories, they are preferable to a schema based on acreage because of the great disparities in both the value and the productivity of land among different parts of the country.

³⁷ The figures are 71.5 percent and 19.4 percent respectively.

TABLE 2
Solvent and Insolvent Mortgagors of the RCB

<i>Category</i>	<i>Number of Estates</i>	<i>Total Valuation in £</i>	<i>Average Valuation in £</i>	<i>Total Mortgage Loans</i>	<i>Average Mortgage Loan</i>	<i>Ratio: Mortgage/ Valuation</i>	<i>Annual Interest Due</i>	<i>Debt Burden %</i>
MAGNATES: Mortgages over £50,000	19	364,127	19,164.6	2,235,899.0	117,678.9	6.1	96,794.8	26.6
“DOUBTFULS”: Interest in Arrear	18	31,712	1,761.8	454,637.5	25,257.6	14.3	19,732.9	62.2

SOURCE: Mortgage Ledgers D1-D3, Y, RCB MSS, Church House, Rathmines, Dublin.

thirty-four small landowners in this sample must have been stretched to the limit to pay so much of their income to the RCB while meeting all their other obligations.³⁸ Solvency for them presumably depended on other sources of income above and beyond their rents. Some may have had a profession and others may have lived off private income. By way of contrast, the average mortgage loan on the twelve great estates was more than fourteen times larger than that on the small estates, yet the interest payments of these magnates came to less than one-fifth of their estimated rental income.³⁹ The richer landowners also enjoyed a lower interest rate than those in the other categories (see Table 1).

The differing degrees of solvency (and insolvency) among the RCB mortgagors come into clearer focus by concentrating on those borrowers who fell behind in their interest payments after 1880. The FC eventually classified these debtors as "doubtful" and wrote off thousands of pounds in both interest and principal shortly before the outbreak of war in 1914. Table 2 compares eighteen of these "delinquent" debtors with the nineteen magnates whose mortgages exceeded £50,000.⁴⁰ Most of the "doubtfuls" were obscure owners of small estates. Their average valuation of £1,761.8 could barely sustain an average mortgage of £25,257.6. Only six of these estates were valued at more than £2,000.⁴¹ The ratio of mortgage principal to valuation—14.3—was high enough to suggest that these owners must have had other assets besides rents, as the FC could hardly have considered this ratio a safe investment. In theory these "doubtfuls" were paying an average of almost 65 percent of their estimated rental income to the RCB. In practice, however, few of them paid the full interest after the early 1880s; and by 1900 some of them owed up to eight years' back interest.

As Tables 1 and 2 show, the actual amount of the mortgage loan mattered far less than the relationship of interest to income; and, for most owners of small- and even medium-sized estates, the burden of debt weighed heavily. In the long run the RCB lost a significant fraction of its investment in Irish land through default and also through the repayment of principal under the land purchase acts in depreciating stocks and bonds rather than cash. By 1930, some forty-three mortgagors had still not completely repaid the principal; and, by 1940, the FC had written off £161,105.8 in both interest and principal for thirteen of these borrowers.⁴² The average debt burden of the "doubtfuls" does indicate that, when mortgagors had to devote more than 60 percent of their rental in-

³⁸ These loans preceded the age of amortization. Had "modern" lending procedures been in effect, only a minority of Irish landowners in the "medium" and "small" categories could have afforded to amortize their debts before or after the Famine.

³⁹ Twelve of the RCB's mortgagors owned "great" estates. Their combined loans amounted to £1,484,365 or 43.8 percent of the RCB's investment in Irish land as of 1880. ML/RCB, D1-D3, Y.

⁴⁰ There were nineteen persons in the "doubtfuls" category, one of whom, William Percy Jones, does not appear in the "Return of Owners of Land in Ireland, 1876." He has, therefore, been omitted from Table 2.

⁴¹ The median average valuation of these "doubtful" estates was £1,382. Four of them were located in Kerry and three in the much richer county of Meath. The reasons for their insolvency remain unknown.

⁴² The RCB's records do not contain summaries of the losses sustained on the investment in Irish estates. The figures presented here reflect occasional entries in the minutes of the FC's meetings, and the total amount written off may well have been much higher than the sums stated here. Before 1914, the FC established a sinking fund in order to cover some of the losses on the mortgages.

come to interest payments, they were heading for irrecoverable arrears, insolvency, and other embarrassments.

The RCB mortgage ledgers do not, unfortunately, provide answers to a number of pressing questions. First, what other mortgages, if any, did these borrowers have, to what amounts, and from which moneylenders? Second, what other sources of income did these landowners possess in addition to their rents? Third, given the costs of operating an estate—including head rents, tithe rent charges, improvements, wages, salaries, pensions, supplies, and subscriptions—how did any heavily incumbered owner manage to survive for more than a few years if he had to spend over 50 percent of his annual income on servicing fixed debts and charges? On many estates after 1850 the normal annual expenditure under all headings except incumbrances and family charges came to at least one-third of the rents received; so it is hard to imagine how owners coped with debts that ate up more than half of their income without resorting to more borrowing. Fourth, these mortgage data reveal nothing about other liabilities. Apart from jointures and portions for younger children that were presumably paid out of the sums borrowed from the RCB, there were personal debts secured by bond and also bank overdrafts, which had to be redeemed sooner or later. It is more than probable, then, that the RCB data represent the bare minimum spent by these landowners on their financial obligations.⁴³

It would be helpful to know, moreover, just how typical of the landed class these RCB mortgagors were. Were they more or less incumbered than all other Irish landowners, and did the FC choose them out of a host of applicants for loans because they were more solvent or because they were prominent members of the Church of Ireland? If there are no precise answers to these questions, the fragmentary evidence suggests that, while the FC cared most about protecting its investments, such criteria as rank and influence may have played some part in deciding to approve or disapprove certain requests for money. Granted that the RCB advanced money to a few of the richest landowners in the country including the Marquess of Downshire and John Mulholland,⁴⁴ some of their mortgagors hardly deserved full confidence. As Table 2 shows, the RCB was prepared to advance to some mortgagors—the “doubtfuls”—loans worth 14.3 times the annual valuation. But the average ratio between mortgage loans and valuation for all one hundred and twenty estates came to 6.4. In a few instances, the ratio fell well below this average—to a mere 3.6 for the Marquess of Downshire’s mortgage of £329,000.

In the case of Anna, the dowager Countess of Kingston, the FC approved a loan of £236,000, which exceeded valuation by a factor of 13.1. Although a cau-

⁴³ Jointures and annuities could account for as much as 10 or 20 percent of rental income. On the Doneraile estate in north Cork, one jointure of £1,000 and pin money of £500 to Lady Doneraile absorbed 22 percent of the rents received (£6,807.5) in 1890. Interest payments of £800 on one mortgage (for £20,000) and some other loans consumed 46.1 percent of all receipts that year. Doneraile MSS, NLI.

⁴⁴ John Mulholland borrowed £120,000 from the RCB in July 1873 and set a record for promptness by repaying the whole amount on January 7, 1881. He may have used this sum to buy new “plant” for his factory, the York Street Spinning Company, in Belfast or to complete the purchase of the Ballywalter Park estate in county Down from his own mortgagor, the Earl of Dufferin, to whom he had loaned £135,346.9 by 1872. ML/RCB, D3. Also see note 68, below.

tious investor might have hesitated before lending money to an estate with such a history of financial as well as emotional instability, the FC apparently did not. Even though the countess was paying out roughly 55.7 percent of her estimated income in interest (£10,006.6 during the 1890s), she did not apparently fall into arrears, and the entire principal was paid off after the sale of fourteen thousand acres of the Cork estate in 1907–08.⁴⁵

What the FC could not have predicted, of course, was the calamitous effect of agricultural depression and agrarian turmoil upon landed property after 1878. These two intimately related phenomena scared off investors and lowered rental income to the point where marginally solvent estates became insolvent.⁴⁶ These estates ended up under receivership in the Land Judges Court or were offered for sale to occupying tenants under the Ashbourne Act and its successors. Aided and abetted by the fair rent clauses of the Land Act of 1881, rental income declined all over Ireland, and this downturn automatically increased the debt burden of incumbered owners. During the early 1880s many landlords wrote distressed letters to their creditors pleading for more time in which to pay the interest due as their tenants had paid little or no rent for months. The FC did call a halt to further mortgages at the height of the land war, but it could do nothing to safeguard those loans already made. The harsh realities of Irish agriculture and agrarian unrest explain why, by 1901, the arrears of interest owed to the RCB reached £134,899—a sum equal to one full year's interest on the £3.1 million invested in mortgages in that year.⁴⁷

THE SECOND POINT OF ENTRY into the problem of landed indebtedness may be called the Land Commission route. As shown earlier, landlords wishing to sell parts of their estates (tenanted land) under the land purchase acts had to submit a host of documents to the Land Commissioners, including a complete list of all outstanding charges or claims on the lands being sold. These Final Schedules of Incumbrances contain extremely valuable information about the various debts and charges that the majority of landlords carried through the nineteenth century.⁴⁸ The results of analyzing the incumbrances in a sample of one hundred and five FSIs may be seen in Table 3, which reveals an average debt burden of 16.3 percent for these vendors of land under the Wyndham Act of 1903.⁴⁹

⁴⁵ For the massive debts of the third Earl of Kingston, who was “of unsound mind,” and the troubles of this estate during and after the Famine, see Donnelly, *Nineteenth-Century Cork*, 70, 113, 255, 278–82, 335, 337–47. Lady Kingston eventually sold 21,417.2 acres or 90.8 percent of her Cork estate under the Wyndham Act.

⁴⁶ Investors' fears about conditions in Ireland after 1878 were voiced by spokesmen for British and Scottish insurance and assurance companies, which had invested several million pounds in Irish mortgages. See Canadine, “Aristocratic Indebtedness,” 646. In Ireland the mortgage market dried up rapidly for a few years during the land war, and needy landowners had a hard time finding loans at less than 6 percent. For a case in point, see page 358, below.

⁴⁷ RCB, *Annual Report*, 1901.

⁴⁸ For the inherent limitations of this evidence, see note 31, above. Whether the land court judge's decision to “disallow” any claim in the FSI meant that the claim itself was invalid or that the claimants had waived their claim or had failed to provide adequate documentation in support of same is not always clear. The FSIs are located in the files for each estate sold by the Estates Commissioners, under the Land Purchase Acts of 1903 and 1909, in the Records Branch of the Irish Land Commission [hereafter, EC/ILC].

⁴⁹ The one hundred and five FSIs here represent something other than a random sample. Not only are they

TABLE 3
FSI Sample of Landlord Indebtedness by Estate Valuation

<i>Estate Valuation in £</i>	<i>Number of Estates</i>	<i>Total Valuation in £</i>	<i>Total Incumbrances in £</i>	<i>Ratio: Mort./ Value</i>	<i>Annual Interest at 4½%</i>	<i>Debt Burden %</i>
SMALL:						
100–1,000	33	12,721.4	84,147.7	6.6	3,786.6	29.8
MEDIUM:						
1,000–5,000	33	92,320.1	475,469.8	5.2	21,396.1	23.2
LARGE:						
5,000–15,000	24	235,715.0	1,107,486.8	4.7	49,836.9	21.1
GREAT:						
Over 15,000	15	368,896.1	907,617.0	2.5	40,842.8	11.1
TOTALS:						
All Estates	105	709,652.6	2,574,721.3	3.6	115,862.4	16.3

NOTE: The incumbrances consist of mortgage loans and marriage portions, exclusive of annuities and jointures.

SOURCE: Estates Commissioners Papers, Records Branch, Irish Land Commission, Dublin.

Like the RCB data, the FSI sample underlines the heavy debt burden carried by owners of small estates. Although the percentage spread between the debt burdens on great and small estates is not as wide in this table as in the RCB sample (18.9 for the FSIs and 51.5 for the RCB mortgages), the relative solvency of the great estates stands out prominently in both.⁵⁰ The FSI data emphasize the truism that “richer is better”—at least where incumbrances are concerned. Owners of small estates had so much less with which to cushion themselves against the adverse conditions of the rural economy after 1878 and before the sale of land became the obvious solution to agrarian discontents.

Even more than the RCB data, these FSIs represent a minimal state of indebtedness, because these FSIs rarely derive from the very first sale of land by an owner and because none of the FSIs covers the entire estate owned by the vendor. At least half of the vendors in this sample may have sold some land be-

too few to constitute an adequate measure but there are proportionately more FSIs for the eastern than for the western counties, which raises the possibility that the data are skewed in favor of more solvent properties.

⁵⁰ In other words, the debt burdens in the RCB sample range from 19.4 percent for the “great” estates to 70.9 percent for the “small,” while the range in the FSI sample is 11.1 percent for “great” to 29.8 percent for “small” estates.

fore this particular sale, and these previous sales would have enabled them to redeem some of their charges or debts. Naturally, any incumbrances discharged at an earlier time would not show up on subsequent FSIs.⁵¹

Despite the artificial nature of most administrative boundaries there is something to be said for classifying landed indebtedness according to region or province. For years economists and agricultural experts have recognized distinctions between the eastern and western parts of the country—roughly, a distinction equivalent to the division between Ulster (excluding Donegal) and Leinster on the one hand and Munster and Connaught on the other. The contrast in resources and wealth between northeast Ulster and western Connaught represents the extreme limit of these regional variations, some of which persist to this day.⁵² Analysis of the mortgage data from our two sources by the province in which the bulk of each mortgagor's property lay shows Ulster as the undisputed leader in terms of solvency or lightness of debt burden. As Tables 4 and 5 indicate, these northern landowners had to divert significantly less of their income to interest payments than did their nearest rivals in Leinster.

The apparent discrepancy between the overall debt burdens of the two samples—27.5 percent for the RCB mortgages and 16.3 percent for the FSI landlords—reflects the basic difference between these sources. The RCB ledgers contain, naturally enough, the names of known mortgagors, whereas the FSIs concern landlords whose lands may or may not have been incumbered, or vendors who may have redeemed their mortgages through earlier sales. Both samples are clearly too small to warrant unequivocal conclusions about the economic condition of all Irish landowners; and the figures in these two tables are slightly skewed by the fact that the largest owners often had estates in more than one province.⁵³ In Table 4 column 7 represents the value of all land owned by the vendor in the province where the sale occurred, and column 8 contains the valuation of all land owned by the vendor in every province. If we assume that the landlords in this sample had no incumbrances other than those charged on the lands being sold (a very risky assumption indeed), then we may decide that column 8 contains a more accurate impression of landlord indebtedness. If, on the other hand, we are concerned with indebtedness by province, then the figures in column 7 should be taken as a rough index of solvency.⁵⁴ In either case the disparity between the debt burdens of the Ulster landlords and those in the

⁵¹ In many cases the FSIs for earlier sales under the Wyndham Act were missing from the Estates Commissioners' files. Some of these missing FSIs may have been extracted and retained by solicitors acting on behalf of the vendor. The Allocation Schedules, which contain much the same information as the FSIs and which seem to have been stored elsewhere in the Land Commission, were available in only a few cases.

⁵² For discussions of regionalism in Ireland, see T. W. Freeman, *Pre-Famine Ireland: A Study in Historical Geography* (Manchester, Eng., 1957); S. H. Cousens, "Regional Death Rates in Ireland during the Great Famine," *Population Studies*, 14 (1960): 55–74, and his two articles on emigration and population change, *Economic History Review*, 14 (1961): 275–88, and 17 (1964): 301–21; and Ó Tuathaigh, *Ireland before the Famine*, chap. 4.

⁵³ This is especially true of Baron Ashtown, Viscount de Vesci, the Marquess of Downshire, the Earl of Longford, and Viscount Massereene, all of whom owned estates in more than one province.

⁵⁴ As Table 4 reveals, the average debt burden of the one hundred and five landlords in the FSI sample comes to 16.3 percent on the provincial basis and to 14.7 percent counting all land owned in Ireland. If annuities, jointures, judgments for debt, and bank overdrafts are included, then the provincial debt burden becomes 20.3 percent and the all-Ireland debt burden rises to 18.2 percent.

TABLE 4
Distribution of FSI Mortgages by Province

<i>Province</i>	<i>Number of Estates</i>	<i>Total Mortgage Loans</i>	<i>Portions for Younger Children</i>	<i>Total Incumbrances in £</i>	<i>Annual Interest at 4½%</i>	<i>Valuation by Province</i>	<i>Valuation of All Land in Ireland</i>	<i>Debt Burden % Pro- vincial Value</i>	<i>Debt Burden % All Ireland Value</i>
CONNAUGHT	13	569,587.9	107,357.4	676,945.3	30,462.5	76,339.3	101,233.9	39.9	30.1
LEINSTER	37	479,410.9	378,810.1	858,221.0	38,620.0	207,839.6	247,564.8	18.6	15.6
MUNSTER	26	148,123.3	203,676.7	351,800.0	15,831.0	77,672.5	82,391.5	20.4	19.2
ULSTER	29	536,092.2	151,662.8	687,755.0	30,949.0	347,801.1	358,248.7	8.9	8.6
ALL IRELAND	105	1,733,214.3	841,507.0	2,574,721.3	115,862.5	709,652.5	789,438.9	16.3	14.7

NOTE: The valuation categories represent, first, the valuation of the mortgagor's estate in the province wherein his principal property lay and, second, the valuation of all of the land owned by the mortgagor in every province.

SOURCE: Estates Commissioners Papers, Records Branch, Irish Land Commission, Dublin.

TABLE 5
Distribution of RCB Mortgages by Province

<i>Province</i>	<i>Number of Estates</i>	<i>Total Valuation in £</i>	<i>Total Mortgage Loans</i>	<i>Ratio: Mort. / Value</i>	<i>Annual Interest Due</i>	<i>Debt Burden %</i>
CONNAUGHT	19	93,587.0	603,854.1	6.5	25,868.4	27.6
LEINSTER	39	152,639.4	1,068,474.3	7.0	45,333.2	29.7
MUNSTER	39	157,482.3	1,111,422.0	7.1	48,714.1	30.9
ULSTER	23	196,379.3	1,028,040.0	5.2	44,695.5	22.8
ALL IRELAND	120	600,088.0	3,811,790.4	6.4	164,611.2	27.4

SOURCE: Mortgage Ledgers D1–D3, Y, RCB MSS, Church House, Rathmines, Dublin.

other provinces, especially Connaught, remains distinctive. The debt burden of the Ulster landlords in the FSI group averages 8.6 percent compared with the 19.7 percent average for those in the other three provinces.⁵⁵ These figures should be treated with the caution due to anything less than an adequate sample. But they do point toward the relative success of these northeastern landlords in managing their estates and their debts more efficiently than did their counterparts in the rest of Ireland.

Table 6 reduces the FSI figures on provincial indebtedness to mean averages (using the all-Ireland basis for valuation). The percentages of debt burden are the same as those in Table 4, but an attempt has been made to distinguish mortgage loans from portions for younger children.⁵⁶ The results of this separation,

⁵⁵ The Ulster sample does contain a higher proportion of wealthy and aristocratic owners than do the other provincial samples. Slightly over half of these Ulster landowners are peers, and the percentage of “great” estates here is 27.6, compared with 15.4 percent for Connaught, 10.8 percent for Leinster, and 3.8 percent for Munster. This bias in the Ulster sample accounts for only a part of the solvency of the estates in this sample. If all of the “great” estates are excluded from the FSI sample and the debt burdens for the remaining ninety owners of estates valued at less than £15,000 are estimated, the Ulster estates still emerge as the most solvent. In ascending order of debt burdens by province the figures are Ulster, 6.5 percent; Leinster, 28 percent; Munster, 29.2 percent; and Connaught, 53 percent. The lowest debt burden in the entire sample—a mere 4.1 percent—belongs to the five Ulster landlords who are in the “medium” category. The lighter debt burden of the Ulster landlords cannot, therefore, be explained away simply by the presence of so many magnates.

⁵⁶ The distinction between ordinary mortgage loans and marriage portions is sometimes blurred, and the incumbrances listed in the FSIs and outlined in the vendor’s Originating Application may have involved family charges without specific indication thereof. In most cases, however, the abstracts of the incumbrances listed in the FSIs contain some clues about the presence or absence of portions for younger children. Mortgages arising out of marriage settlements usually point to portions and contingency jointures. Unfortunately, the FSIs remain silent about the causes of mortgage loans for nonfamilial purposes. In Table 6 the last column refer-

TABLE 6
Mean Average Debt Burden of FSI Landlords by Province

Province	Number of Estates	Mortgage Loans in £	Portions for Younger Children	Total Incumbrances in £	Annual Interest at 4½%	Valuation by Province	Valuation of All Land in Ireland	Debt Burden % All Ireland Valuation	Average Outlay on Other Debts and Charges
CONNAUGHT	13	43,814.5	8,258.3	52,072.7	2,343.3	5,872.3	7,787.2	30.1	177.5
LEINSTER	37	12,957.1	10,238.1	23,195.2	1,043.8	5,617.3	6,690.9	15.6	346.9
MUNSTER	26	5,697.1	7,833.7	13,530.8	608.9	2,987.4	3,168.9	19.2	121.0
ULSTER	29	18,485.9	5,229.8	23,715.7	1,067.2	11,993.1	12,353.4	8.6	330.3
ALL IRELAND	105	16,506.8	8,014.4	24,521.2	1,103.5	6,758.6	7,518.5	14.7	265.4

NOTE: In the last column, the figures represent the average annual outlay on other debts and charges, including payments on annuities, jointures, bank overdrafts, legacies, and judgments for debt.

SOURCE: Estates Commissioners Papers, Records Branch, Irish Land Commission, Dublin.

however imprecise, reveal that marriage settlements cost the landowners of Munster and Leinster more (57.9 percent and 44.1 percent, respectively, of total incumbrances) than those in Ulster and Connaught (32.7 percent and 15.9 percent, respectively). Perhaps the incumbered owners in Connaught could not afford generous marriage settlements owing to the heavy debts they had inherited. The Ulster landowners in this sample seem to have kept a tighter rein on marriage portions than did their counterparts in Leinster and Munster. The fact that the Ulster sample contains more magnates or wealthy peers than do the other provincial samples (witness the high average valuation of the Ulster owners in Table 6) may go far to explain the light debt burden of these northern landlords. But such distinctive features of the northeast as systematic improvement of land, efficient estate management, close ties to the linen textile industry, and the widespread recognition of tenant right or the tenant's interest in his own holding must also be taken into account as reasons for the superior economic performance of the Ulster landlords in this sample.⁵⁷

UP TO THIS POINT the extent of landed indebtedness has been hypothesized by equating valuation with rental income and by assuming that these landowners had no other debts or sources of income apart from those indicated. Such assumptions can and do mislead, especially where the income factor is concerned. What about the realities? Do the actual rentals and accounts of Irish estates confirm or contradict the debt burdens estimated above? A partial answer to this question derives from the records of ten estates located in various parts of the country.⁵⁸

The mean average debt burden on these estates comes to 27 percent and the rental income factor places three of them in the "great" category and five in the "large."⁵⁹ Contrary to the results obtained in the RCB and FSI samples, the debt burdens of the four Ulster estates in this group surpass those of the other six by a margin of 33.7 percent to 18.8 percent. But the Duke of Abercorn's heavy debt burden (40.2 percent) accounts for much of this disparity, and, if his liabilities are eliminated, the debt burden of the three remaining Ulster land-

ring to the "Average Annual Outlay on Other Debts and Charges" represents an assessment of 4.5 percent interest on all contingency jointures, annuities, judgments for debt, bank overdrafts, and legacies. The additional annual outlay on these other liabilities comes to a total of £27,869.9, and, by combining this burden with the amount spent on servicing the mortgage loans, the total debt burden becomes 18.2 percent for the landlords in this sample. EC/ILC.

⁵⁷ The Ulster estates had lighter debt burdens in every category of wealth but one: in the "great" estates category, the four Leinster magnates in Table 6 paid an average of 6.7 percent of rental income on interest, compared with 10.5 percent for the eight Ulster magnates. EC/ILC.

⁵⁸ For permission to consult and publish the rentals and accounts needed to construct Table 7, I would like to thank the owners or trustees of the Antrim, Clonbrock, Crofton, Bellew, Inchiquin, Ormonde, Ranfurly, Stewart, and Wheeler-Cuffe papers. In this connection special thanks are due to the Hon. Bryan Bellew, the Hon. Grania O'Brien, and Captain A. C. Tupper. Lord Bellew's interest payment here is a net figure. In this year he earned £1,012.2 in interest on one mortgage loan, and he paid out £2,042.5 on his own loans. Bellew MSS, Barmeath Castle, Dunleer, county Louth.

⁵⁹ The average valuation of these ten estates—£12,704.8—exceeded average rents received in the years indicated—£12,563.2—by only 1.1 percent.

TABLE 7
Indebtedness on Ten Estates

Name of Owner	Owner's County Seat	Valuation of Estate in £	Rents Received in £	Date of Rental	Interest Paid on Mortgages	Est. Amount of Mortgages at 4½%	Debt Burden %	Other Charges and Loans
ABERCORN	Tyr.	35,802	35,936.5	1886-87	14,447.3	321,051.1	40.2	0.0
ANTRIM	Ant.	20,910	15,790.0	1875	4,273.8	94,973.3	27.1	2,782.2
BELLEW	Lou.	5,093	3,166.9	1905	1,030.3	22,895.6	32.5	0.0
CLONBROCK	Gal.	11,442	10,425.2	1880-84	395.4	8,786.7	3.8	0.0
CROFTON	Ros.	6,386	5,953.8	1865	1,911.7	42,482.2	32.1	0.0
INCHQUIN	Cla.	11,681	13,691.5	1878-87	4,548.8	101,084.4	33.2	1,173.5
ORMONDE	Kilk.	16,357	21,168.3	1880-84	2,525.3	56,117.8	11.9	178.1
RANFURLY	Tyr.	11,237	10,772.8	1902	1,693.8	37,640.0	15.7	32.5
STEWART	Tyr.	6,752	6,841.9	1876	2,968.2	65,960.0	43.4	400.0
WHEELER-CUFFE	Kilk.	1,388	1,885.0	1871	185.0	4,111.1	9.8	500.0
TOTALS		127,048	125,631.9		33,979.6	755,102.2	27.0	5,066.3

NOTE: I have estimated all mortgages at 4.5 percent interest, even though some of the owners were paying closer to 4 percent interest on their loans.
SOURCE: See note 58, above.

owners falls to 26.8 percent.⁶⁰ Of course, this sample is far too small to undermine the hypothetical debt burdens found in Tables 4 and 5. Although the average debt burden of these ten estates does come close to the averages in the RCB and FSI samples, the data in Table 7 emphasize the tentativeness of the provincial estimates. In several cases, the dimensions of indebtedness turn entirely upon the date of the sample: a landowner could be relatively unincumbered at one point in time and then find it necessary, especially after 1880, to borrow large sums of money in order to compensate for huge arrears of rent.⁶¹ The data in Table 7 also point up the danger of averaging small samples: the massive debts of only one or two landowners can easily distort the debt burdens of several dozen modest mortgagors in the same cohort.⁶² There were, after all, some prudent Irish landlords who defied the popular stereotype by living within their limited—or diminishing—means.

The records of individual estates contain a spectrum of solvency, marginal affluence, and rapidly approaching insolvency. The difficulty lies in finding the typical estate for any particular region, time, and cohort. Starting with extremes of solvency, one looks in vain for significant incumbrances on the vast Fitzwilliam estate surrounding Coollattin in county Wicklow, on Sir John Leslie's estate at Glasslough, county Monaghan, or on the Earl of Erne's "great" estate in county Fermanagh.⁶³ The distinct advantage of owning urban property becomes apparent in the case of the Earl of Longford, whose agricultural rents were supplemented and then bolstered by rising income from the suburban estate in Kingstown (Dun Laoighaire), which he owned jointly with Viscount de Vesci. The combination of a substantial income in the 1870s—averaging £21,262.8 during 1870–74—and the termination of a long-lived and costly jointure in 1880 enabled Lord Longford to reduce his debt burden from 31.2 percent in the early 1870s to a mere 6.7 percent in the early 1880s.⁶⁴

⁶⁰ According to the Duke of Abercorn's agent, who provided a summary of the "Permanent Charges" on the estate in 1886, "estate charges" cost the duke £9,371.6 and "family charges" amounted to £14,447.3 in that year. At 4 percent interest the principal of the latter would have been £361,181.3. Tithe-rent charges, charges on Church lands, and a Board of Works loan for land improvement have been excluded. Interest on family charges ate up 40.2 percent of the duke's rental income in 1886–87. Between 1886 and 1890 he sold 66.1 percent of his Donegal estate and 27.5 percent of his Tyrone estate for a total of £279,008, and he used this money to pay off most of these charges. By 1889, the duke's debt burden could not have been much more than 13 percent of rents received (£25,235.6). Abercorn MSS, PRONI, D. 2400/1/60; and *PP*, 1889, 61, no. 81, and *PP*, 1890, 60, no. 115. Sir John Marcus Stewart of Ballygawley Park, county Tyrone, had twenty-two different charges or mortgages on his estate in addition to a jointure of £400 paid to Lady Elizabeth Stewart. In 1877 his debt burden, excluding the jointure, came to 43.4 percent of rents received (£6,841.9). PRONI, D. 1716/3A–5A.

⁶¹ Lord Ranfurly's interest payments and annuities, for example, came to only 1.3 percent of rental income (approximately £13,000) in 1872. But, when he sold most of his Tyrone estate in 1904–05, the accounts revealed mortgages totaling £38,500. Interest payments thereon amounted to 15.7 percent of receipts in 1902. Ranfurly MSS, PRONI, D. 1932/4/33–34.

⁶² If the three most heavily incumbered owners are excluded from this sample (their average debt burden was 38.9 percent), the debt burden of the remaining seven falls to 17.4 percent.

⁶³ On Lord Fitzwilliam's estates in Wicklow, Wexford, and Kildare, the agent paid out a mere £633.8 in interest and annuities during 1885–86. The 89,891 acres of this estate were valued at £46,444. Fitzwilliam MSS, NLI. Leslie's Glasslough estate in county Monaghan was valued at £11,540, and his Pettigo estate on the border of Donegal and Fermanagh at £4,126. He owned a total of 49,968 acres in six counties (almost all in Ulster), valued at £21,051. In the 1870s he was paying out around £25 in interest (on only one loan).

⁶⁴ In 1844–45, Edward, the third Earl of Longford, borrowed £50,000 to cover marriage portions and other expenses. The interest payments of William, the fourth earl, fell from £2,948.2 in 1874–75 to £267.2 in 1876–

On the opposite end of the spectrum could be found hundreds and, after 1880, thousands of insolvent estates in receivership. Hard times forced the owners of heavily incumbered estates to enter the land market at a time when prices were low and mortgage money scarce. The number of estates in Ireland over which receivers were appointed by court order rose dramatically from one hundred and sixty-five in 1881 to six hundred and eighty-nine in 1886. Between 1883 and 1887 the Land Judges Court, which had inherited the functions of the Incumbered Estates Court, approved the sale of four hundred and six of these estates for the sum of £1,406,023.6—the average price being £3,463.1.⁶⁵ In the absence of the internal records of these financially stricken estates one can only surmise that the lack of adequate credit as well as a crushing debt burden resulted in the high casualty rate among small estates during a time of depression and agrarian unrest.

In the middle range of marginal solvency could be found landlords like Sir William Verner, third baronet (1856–86), of Church Hill, Verner's Bridge, county Armagh. Sir William inherited 24,144 acres in four counties, valued at £12,820, along with a mortgage of £10,000 and marriage portions to the sum of £18,000. The poorer tenants on the Armagh and Tyrone estates could no longer pay the full rent due after 1879, and, as the arrears mounted, so too did the anxieties of creditors to whom Sir William owed money. His long-suffering agent, James Crossle, spent much time in the early 1880s searching for a mortgage loan of some £60,000 at 4.25 percent or 4.5 percent, presumably to stave off the foreclosure of other mortgages. Evidently the Dublin moneylenders considered this estate a poor risk because they refused to lend money at less than 5 percent. By 1882, several potential creditors were talking 6 percent. Eventually, the agent and the family solicitor in London found someone prepared to advance £60,000, but the struggle to pay bills and meet interest payments on time continued, and Lady Verner's demands for more remittances from the agent did not help matters. The cumulative strain of trying to keep this estate solvent took its toll on Sir William's health and may well have hastened his death in 1886 at the age of thirty.⁶⁶

77. Georgiana, dowager Countess of Longford, survived her husband, Thomas, second earl (who died in 1835) by almost forty-five years. At £2,000 per annum, hers was a costly jointure by almost any standard. Selina, the widow of William, the fourth Earl of Longford, also received a jointure of £2,000 after his death in 1887. Good grazing lands in Longford and Westmeath, efficient management by a professional land agency in Dublin, and receipts from the Kingstown estate kept Lord Longford's rental income at an average of £21,199.3 during 1881–85. Longford MSS, Tullynally Castle, Castlepollard, county Westmeath.

⁶⁵ These were mostly small estates burdened by heavy arrears as well as incumbrances. The one hundred and sixty-five insolvent estates in 1881–82 had an average rental of £599.4, and the six hundred and eighty-nine estates in this category in 1886–87 had an average rental of £513.9. In this latter case the rents received in 1887 represented 61.2 percent of the rents due—a figure expressive of mountainous arrears. "Return to an Order of the House of Commons . . . relating to the Landed Estates Court, Land Judges Branch of the Court of Chancery (Ireland)," *PP*, 1888, 83, no. 400. Also see "Returns of Purchase Money Realised by Sale of Estates in the Landed Estates Court, 1870–79," *PP*, 1880, 60, no. 303. A government return, dated February 1890, contains the names of over two thousand properties "under the control" of the Land Judges Court since January 1, 1878. *PP*, 1890, 60, no. 74.

⁶⁶ See *Copy Out-Letter Books of James and Henry Crossle, Agents for the Verner Estate in Cos. Armagh, Tyrone and Monaghan, 1857–1886* (issued by the PRONI; Belfast, n.d. [1975]). On May 2, 1882, James Crossle, Sir William's land agent, wrote to the Verner's London solicitor. "We are very uneasy about the estate payments; if we cannot meet the interest of the mortgages they will be foreclosed. Last year we were able to borrow 'untill the rents

Several great estates also foundered in a sea of debts after the late 1870s. But these may be instances of exceptions proving the rule. The earls of Lucan had managed to run up mortgages amounting to some £246,258 charged on their vast estate in county Mayo, and only extensive sales of land in 1905 extricated the family from a debt burden approaching 85 percent of estimated income.⁶⁷ Then, too, there is the saga of the first Earl and Marquess of Dufferin and Ava, who inherited an estate in north county Down valued at £16,752. Dufferin was no hedonist or gambler but an ambitious and thoughtful politician with Whigish leanings. In pursuit of high office and ambassadorial pomp, and anxious to be known as an improving landlord, he began to overspend his income by thousands of pounds during the 1860s.⁶⁸ When he took over the estate in 1847, incumbrances came to £29,261.5. By 1862 the estate had been charged with loans to the amount of £142,157.5, and ten years later this figure reached the high-water mark of £304,824. By 1872, Lord Dufferin had exhausted his income and most of his credit. Gladstone's tamperings with landlords' rights in 1870 made Dufferin all the more nervous about his future as a heavily incumbered landowner. With average receipts in the early 1870s of £21,181.2, Dufferin was paying close to £13,717.1 in interest (reckoned at 4.5 percent) plus the sum of £4,115.6 in annuities and jointures to family members. His total debt burden must have come close to 84.2 percent.⁶⁹ Having seen the writing on the wall, Dufferin decided in 1874 to sell the bulk of his estate in the Landed Estates Court. Sales began soon after, and by 1880 he had realized £370,042 in purchase money—a sum that enabled him to discharge all of his incumbrances while leaving a residue of £54,850, most of which he invested in Canadian property.⁷⁰ Lord Dufferin was one of the very few great landowners to sell his estate well before the era of land purchase acts. The staggering size of his debt burden explains why his decision was not wholly voluntary. If he succeeded in escaping from the crushing burden of those debts, he did not elude disputes over tenant right with some of his former tenants.⁷¹

came in' but this year it could not be managed either from banks or private individuals. The charges upon this estate now (particularly in such very unfortunate times) are very heavy and . . . the expenditure is far in advance of the incomings." PRONI, D. 236/488/3, 112.

⁶⁷ Excluding a land improvement loan of £5,700 and a contingency jointure of £2,000, Lord Lucan's incumbrances amounted to a total of £240,557.9, of which marriage portions accounted for roughly £34,666.6. The valuation of his lands in Mayo and Dublin came to £13,119.5. Figuring interest at 4.5 percent, his debt burden on the mortgages would have been some 82.5 percent. The sale of Lucan's Mayo estate in 1905 realized £102,370.8, plus a bonus of £12,284.5, almost all of which went to pay off claimants. FSI, E.C. 656 and C.D.B. 1146, EC/ILC.

⁶⁸ Lord Dufferin claimed that he had spent over £100,000 on improvements to his estate, but there is no proof that this was the real cause of his financial embarrassment. See Sir Alfred Lyall, *The Life of the Marquis of Dufferin and Ava*, 1 (London, 1905): 185, 192. For Dufferin's indebtedness to John Mulholland, see note 44, above. Dufferin was a keen student of land law and wrote memoranda as well as pamphlets about landlord-tenant relations in Ireland.

⁶⁹ These figures derive from the Dufferin accounts, PRONI, D. 1071A/C3/39-40, D. 1071A/N4, and D. 1071A/B4/1. I am most grateful to Andrew Harrison and Patrick Geary for drawing these documents to my attention.

⁷⁰ For particulars of these sales, see PRONI, D. 1071A/C3/40. Five wealthy businessmen from the Belfast area, including John Mulholland, bought most of the estate.

⁷¹ For Dufferin's painful disputes with some of his former tenants over compensation for improvements to their holdings, see Lyall, *Life of the Marquis of Dufferin*, 188-201. Dufferin expressed his views on landlord-tenant relations in a pamphlet, *Contributions to an Inquiry into the State of Ireland* (London, 1866).

If space permitted, many other case histories could be cited showing how hard-pressed landowners tried to cope with their debts. But the purpose here is to generalize not particularize. Although the mortgage data and the estate accounts remain silent about the reasons for all this mortgaging, the scattered evidence does suggest that once the economy had recovered from the worst effects of the Famine, landowners already burdened with family charges and other liabilities jumped at the chance offered by both private and institutional lenders, themselves in search of a good investment, to consolidate all of their debts in one giant loan at rates ranging from 4 to 4.5 percent.⁷² In this way an incumbered owner could pay off the old incumbrancers and greatly simplify his interest payments to the new creditor who had a first charge on the lands in question. No doubt the hard times after 1878 drove many owners to seek loans that would permit them to maintain their affluent life-style. But when creditors began to shy away from investing in Irish land during the land war, incumbered owners had a hard time finding mortgages at less than 5.5 or even 6.5 percent.

Although distinctions between ordinary mortgage loans and family charges are often difficult to draw, the FSI's do contain some clues about the role of marriage settlements in causing indebtedness. Roughly one-third of the incumbrances redeemed at the time of sale involved portions for younger children, and the proportion may well have been higher than the figures shown in Tables 4 and 6.⁷³ If this estimate applied to the rest of the landowning class, then the amount of capital tied up in this way must have run into millions of pounds. Had these portions and jointures been invested in productive capital ventures, the landlords of Ireland might not have been so vulnerable to rent strikes and falling agricultural prices. But in the long run most landlords cared more about preserving status than maximizing profit. Excepting such fortunate owners of Dublin city property as Lords Pembroke, Meath, Carysfort, de Vesci, and Longford,⁷⁴ the majority of landlords were forced to reduce their personal expenses

⁷² Among the institutional lenders in Ireland, besides the RCB and various banks, were the Presbyterian Church, which had advanced £121,000 to the ninth Earl of Shaftesbury by 1872, and St. Patrick's College, Maynooth, which loaned a total of £314,670.2 to seven mortgagors in the 1870s. See PRONI, D. 811/257/1; and Mortgage Papers, Maynooth. British insurance companies loaned several millions to Irish as well as English landowners during the nineteenth century. For example, in 1847 the Law Life Assurance Society allowed a mortgage for £160,000 to Thomas Martin of Ballinahinch and a few years later "bought in" this insolvent estate in Connemara for £186,000. By 1894 the Earl of Lucan owed £191,969.8 to both the Law Life and the Eagle Life Assurance companies. By 1902, Lord Charlemont had borrowed £205,000 from the Royal Exchange Assurance Company. And so on. See Padraig G. Lane, "The Management of Estates by Financial Corporations in Ireland after the Famine," *Studia Hibernica*, 14 (1974): 67-89; FSI (Lucan), E.C. 656, EC/ILC; and Cannadine, "Aristocratic Indebtedness," 637.

⁷³ Gerald, fifth Duke of Leinster, inherited in 1887 incumbrances totaling almost £292,077, at least half of which consisted of portions for younger children (approximately £154,000). The pressure of these and other debts moved the trustees to sell £256,204 worth of the county Kildare estate under the Ashbourne Act (1885-91). The remainder of the Kildare and Meath estate, excluding Carton demesne and some lands in hand, was sold in the early 1900s for a grand total of £676,038. *PP*, 1889, 61, no. 81: 12; *PP*, 1890, 25, C. 6202 and 60, no. 115; *PP*, 1890-91, 25, C. 6384 and C. 6508; *PP*, 1907, 70, Cd. 3447, Cd. 3547, and Cd. 3531. Also see FSI (Leinster), E.C. 82-85, EC/ILC.

⁷⁴ The combined valuation of the Dublin property owned by these five peers amounted to £82,857, but this figure may well mislead because income from the ground rents steadily increased after 1870. Gross receipts on Lord Pembroke's great urban estate in south Dublin came to £31,835 in 1870, £35,339.3 in 1880, and £36,923.5 in 1890. Pembroke Rentals, PRO, Dublin.

rather severely after 1878, and almost all of them curtailed the already modest sums being spent on improvements to the estate.⁷⁵

Landowners who succeeded to estates after the 1850s, and especially those stirred by evangelical imperatives, tended to act more frugally than their fathers and grandfathers had done. In the post-Famine era many owners worked hard to make their home farms as well as their estates pay. They could usually rely on more efficient or less unscrupulous land agents to help them achieve this end, even though the quality of agents varied widely between the amateur, feckless nephew or cousin of the owner and the highly professional land agency firm with headquarters in Dublin or Belfast. What the landlords did not count on was the steady erosion of deference among their tenants and the fall in agricultural prices during the 1870s, two processes that culminated in the land war of 1879–82. The rapidity and severity of the agricultural crisis caught many landowners by surprise, particularly those who had embarked on ambitious projects to expand demesne farming, modernize the Big House, or enhance the pleasure grounds. In the easy credit situation of the 1860s and early 1870s landowners had no trouble in financing these ventures through loans. Then came the succession of bad harvests, falling prices, declining rental, agrarian agitation directed at landlordism, a tight mortgage market, and defaults on mortgage payments. The dramatic rise in the number of estates passing from solvency to insolvency in the 1880s bears mute but irrefutable witness to the impact of these blows on Irish landed society. For landowners saddled with the heaviest debt burdens the day or month of reckoning came that much sooner and with painful finality.

At the end of his article on Irish landed wealth, David Large raised an important question about the timing and, hence, the motivation of the debts incurred by the few landowners in his sample. Although lacking statistical evidence, he speculated that, contrary to Tocqueville's view about heavy borrowing between 1815 and 1849, "indebtedness was a serious matter for many landowning families" before 1815.⁷⁶ The evidence gathered for the present study would suggest a different conclusion or set of inferences. The chronology of indebtedness herein shows a distinct preponderance of mortgage activity after, rather than before, the Famine. No doubt many landlords had to sell some or all of their estates in the Incumbered Estates Court simply to avoid bankruptcy proceedings. But the FSIs reveal a series of strenuous efforts dating from the 1850s through the 1870s to refinance family charges and other liabilities contracted in more prosperous

⁷⁵ The amounts spent on agricultural improvements by Irish landlords varied widely. To confuse the issue some agents tended to count as improvements money spent on the gardens or pleasure grounds, plantations, and game-keeping. For political reasons many landlords exaggerated the size of their investment in capital improvements to the estate. Analysis of the accounts of four estates (Cosby in Queen's county, Dunraven in Limerick, Longford in Westmeath and Longford, and Crofton in Roscommon) reveals the following percentages of rental income spent on permanent improvements between the 1860s and the 1880s: 9.6 percent, 8.8 percent, 24.8 percent, and 6.9 percent, respectively. Cosby MSS (Stradbally); Dunraven MSS (now on deposit in the PRONI); Longford MSS (Tullyally Castle); and Crofton MSS (NLI). Much more work needs to be done on estate accounts before any firm conclusions can be drawn about the average expenditure by Irish landowners on this important item.

⁷⁶ Large, "The Wealth of the Greater Irish Landowners," 43–44.

TABLE 8
The Chronology of Indebtedness:
Distribution by Decade of 270 Mortgages

<i>Decade</i>	<i>Number of Mortgage Loans</i>	<i>Amount of Mortgage Loans</i>	<i>Average Size of Loan</i>	<i>Percent of Total Mortgage Loans</i>
1770-79	2	18,000.0	9,000.0	0.9
1780-89	3	25,115.4	8,371.8	1.3
1790-99	5	51,600.0	10,320.0	2.6
1800-09	1	6,261.5	6,261.5	0.3
1810-19	7	101,078.3	14,439.8	5.1
1820-29	4	44,000.0	11,000.0	2.2
1830-39	13	143,120.6	11,009.3	7.2
1840-49	16	203,130.7	12,695.7	10.2
1850-59	21	268,066.5	12,765.1	13.5
1860-69	38	378,727.6	9,966.5	19.1
1870-79	38	269,869.6	7,101.8	13.6
1880-89	29	119,475.6	4,119.8	6.0
1890-99	48	180,328.0	3,756.8	9.1
1900-09	37	151,445.6	4,093.1	7.6
1910-19	8	26,200.0	3,275.0	1.3
TOTALS	270	1,986,419.4	7,357.1	100.0

SOURCE: See note 77, below.

times. In a number of cases the marriage settlements of the 1820s and 1830s fell due for payment—in the form of portions and jointures—after 1850. And, even if landowners did run up some huge debts before 1815, there can be no doubt about the zeal with which the landlords in this sample borrowed money after 1850. What remains unclear is how much of a role necessity rather than desire or vanity played in these transactions.

Table 8 shows the distribution by decade of two hundred and seventy separate mortgage loans culled from the FSIs of landlords in Leinster, Munster, and Connaught.⁷⁷ As can be seen, some 70.2 percent of all of these borrowings took place in the post-Famine period, whereas only 10.2 percent date from the last third of the eighteenth century. The extent of mortgaging in the thirty years after the Famine—46.2 percent of the total—may reflect a tendency to raise fresh capital or refinance old debts after a time of agricultural depression and when there were signs of stability or prosperity close at hand. The absence of concrete evidence about why these landlords borrowed when they did makes it necessary to seek clues among the skeletal remains of once vital and complex indentures of mortgage, the full circumstances of which can only be a matter of guesswork or conjecture.

MANY QUESTIONS ABOUT THE ORIGINS, nature, and effects of landed indebtedness in Ireland remain either unposed or unanswered in this study. Had more evidence survived, for example, it might have been possible to estimate the number of landowners who were free from incumbrances or mortgages. All that can be said on the basis of the FSI sample is that only sixteen of the one hundred and five landlords in this group (15.2 percent) had no mortgages listed against the lands being sold at that time.⁷⁸ Such unincumbered FSIs may, of course, reflect the fact that the vendor had sold land previously and had used the proceeds to discharge his outstanding debts. Even given the inherent limitations of the evidence, the mortgage data used in this study do point toward several conclusions.

First, the great or wealthiest estates seem to have carried on balance the lightest debt burdens of any category of owner regardless of the actual size of the mortgages. Conversely, the most heavily incumbered estates tended to be those with a net rental of less than £1,000. Because this study has focused on the Irish landlord class as a whole and not just on aristocratic owners, it is not possible to enter fully into the debate about the nature of aristocratic indebtedness in England, carried on in a “genteel” fashion by David Spring, F. M. L. Thompson,

⁷⁷ The data in Table 8 come from the FSIs for the vendors in the sample of these three provinces; EC/ILC. No distinctions have been made between old and new pounds. This sample of two hundred and seventy mortgages and charges contains fifty-three identifiable marriage settlements that provided portions to the amount of £542,605, or 27.3 percent of the face value of all of these incumbrances. But many of the other mortgages in this sample may have arisen out of earlier marriage settlements not listed in the FSIs sampled here.

⁷⁸ The Earl of Antrim, Baron Leconfield, the Earl of Carysfort, the Earl of Belmore, the Earl of Lanesborough, and the Marquess of Londonderry had no mortgage loans listed in their FSIs in this sample. When Lord Leconfield sold almost 95 percent of his county Clare estate in 1894, his FSIs contained no incumbrances. ILC 694C, EC/ILC.



Figure 1: The evils of landlordism. The nationalist cartoonist, Thomas Fitzpatrick, depicted a rapacious Anglo-Irish landlord steering the Tory government of Lord Salisbury toward another campaign of eviction and repression in Ireland. The torn and tattered coat, breeches, and boots are meant to suggest heavy debts or lack of rental income rather than simple sartorial neglect. Fitzpatrick's cartoon appeared in the *Weekly Freeman*, December 4, 1886.

and, more recently, David Cannadine.⁷⁹ The presence of an aristocratic element in the mortgage samples, however, makes it possible to assert that owners of “great” estates in Ireland behaved in most respects like their analogues in England: huge debts inherited from extravagant forebears were supported effectively by huge incomes. Strategic sales of land and occasionally shrewd investments in business ventures enabled these magnates to retain their wealth and status long after the lesser landowners with their heavier debt burdens had sold everything except the demesne and the family silver. As in England, so in Ireland, the wealthiest landowners could afford to live with mortgages and family charges in excess of £200,000—a burden that would have crushed a landowner worth even £10,000 a year. Admittedly, a severe depression, an imprudent marriage to an “actress,” or a passion for gambling could jeopardize a great estate, but only if the debt burden was already onerous.⁸⁰ In spite of the pronounced differences between England and Ireland, Cannadine’s reflections about English landowners apply with almost equal force to Irish landed society:

⁷⁹ Cannadine has summarized the main points in this debate and provided the bibliographical background; see his “Aristocratic Indebtedness,” 624–50.

⁸⁰ An example of the ruinous effects of this combination of circumstances may be found in the case of Edward, seventh Duke of Leinster (1892–1976), who gambled away a fortune he did not expect to inherit (being the third son of Gerald, fifth Duke of Leinster). Around 1919 he sold his reversionary interest in the much-reduced estate to the English clothier and moneylender, Sir Harry Mallaby-Deeley, in order to raise some cash. When Edward finally succeeded to the estate and the title on the death of his eldest brother, Maurice, the sixth duke, in 1922, Sir Harry became the legal owner of the Carton property. W. A. Maguire has studied another aristocratic gambler’s “progress” and the high cost of high living in an earlier period; see Maguire, “The 1822 Settlement of the Donegall Estates,” *Irish Economic and Social History*, 3 (1976): 17–32.

Retrenchment, sales of land, new or enlarged debts, the abdication of traditional paternal and political roles—all of these activities characterized aristocrats of moderate means in the years from the 1870's. For the squirearchy and minor gentry beneath—those with incomes from £1,000 to £10,000 a year—the pressure of debt was greater, the impact of depression more marked, and decline in consequence more rapid and complete.⁸¹

From an Irish perspective this argument requires at least one important qualification: “ruinous debt,” as distinct from tolerable debt burdens, played a far more active role in deciding the fate of Irish landlords outside Ulster than was the case in England.⁸²

The unequal distribution of wealth or debt burdens in Ireland goes some way to explain why so many small landlords could not afford to be too lenient toward their tenants' arrears and also why they usually had to back down before Land League threats of rent strikes and grant large abatements on the rents due after 1879.⁸³ These smaller and poorer landlords, moreover, had to sell more land sooner than they may have wished, and, if they did not sell voluntarily, the Land Judges Court was likely to appoint receivers and conduct the sales for them.

Second, there can be little doubt that mortgage loans and family charges weighed heavily on many Irish landlords and that the misfortunes of agriculture in the latter 1870s and 1880s drove many marginal estates into insolvency. Excepting the great estates and those landowners who were fortunate enough to own urban property or coal mines, the extent of this indebtedness runs directly counter to James S. Donnelly's optimistic assumption that the operation of the Incumbered Estates Courts after 1850 relieved the landlords of a “major problem”—indebtedness—and freed them to invest fruitfully in estate improvements.⁸⁴ The court certainly did disincumber landlords with the heaviest debt burdens in the 1840s and after, but the redistribution of almost five million acres under its auspices could not possibly have alleviated the problems of those owners who were caught up in the struggle to stay solvent.⁸⁵ As the RCB and FSI samples reveal, many landlords carried the burden of pre-Famine incumbrances right through the nineteenth and well into the twentieth century. And that burden became a “major problem” in the years after 1878 for landlords who were already spending upwards of 40 percent of their income on in-

⁸¹ Cannadine, “Aristocratic Indebtedness,” 648.

⁸² Cannadine has concluded that “the distinction between early Victorian crisis and mid-Victorian recovery has perhaps been overdrawn; and . . . debt, but not ruinous debt, was a common feature of life for many [English] landed families.” *Ibid.*, 649.

⁸³ Both the Land League and the Plan of Campaign organized rent strikes and other forms of agitation on estates known to be heavily incumbered and whose owners could not afford to hold out long (at least not without the help of an owners' syndicate) against tenants who withheld the rent. See L. P. Curtis, Jr., *Coercion and Conciliation in Ireland* (Princeton, 1963), 238–41, 248–52; and F. S. L. Lyons, “John Dillon and the Plan of Campaign, 1886–90,” *Irish Historical Studies*, 14 (1965): 313–47.

⁸⁴ Donnelly's rather rosy view of the landlords' post-Famine adjustment deserves quoting: “The incubus of a largely bankrupt or debt-ridden landlord class was cast off by the operation of the incumbered estates court in the 1850s”; *Nineteenth-Century Cork*, 164. Also see Cannadine's comments on Donnelly's argument; “Aristocratic Indebtedness,” 633, n. 6.

⁸⁵ For the operation of the Incumbered Estates Court, see John E. Pomfret, *The Struggle for Land in Ireland, 1800–1923* (Princeton, 1930), 43–46; and Padraig G. Lane, “The Encumbered Estates Court, Ireland, 1848–1849,” *Economic and Social Review*, 19 (1972): 413–53.

terest and other charges.⁸⁶ When one adds to the mortgages all of the other compulsory payments faced each year by the landlords (head rents, crown rents, quit rents, tithe-rent charges, county cess, and poor rates), then the amount of "profit" in the "profit rent" shrinks all the more. Not until the passage of the land purchase acts after 1885 did these incumbered owners find a way out of a threatening situation, and even then their spokesmen in the House of Lords clamored for some kind of tangible compensation to atone for all their losses between 1880 and 1900.⁸⁷

Third, the analysis of the mortgage data suggests that Ulster landlords on the whole enjoyed a greater degree of solvency than those in the other provinces. For various reasons, including improvement of farm land, less troubled landlord-tenant relations, and their proximity to the flourishing linen textile, shirt-making, and ship-building industries, these northern landlords had more liquidity or capital at their disposal. In their politics and religion they were less alien members of provincial society than their counterparts in Munster or Connaught, and many of them had social as well as business ties to the commercial and industrial magnates of Belfast and other northern cities. The greater wealth of these northern landowners enabled them to invest in business ventures and to maintain their social and political ascendancy in the countryside.

Relatively unincumbered wealth made it possible for Ulster aristocrats like Lords Londonderry, Belmore, Erne, Downshire, Farnham, and Ranfurly to preserve their privileged position no matter how much land they may have sold after 1885. Landlords in the "south," on the other hand, were retreating on all fronts before the forces of democratic and Catholic nationalism. Prominent in leading as well as financing such antinationalist organizations as the Property Defence Association and the Irish Unionist Alliance, these northern landlords also helped to launch and supervise the Ulster Volunteer Force in 1912-14.⁸⁸ Ulster Unionism, in short, represented a formidable coalition of landed wealth, industrial-commercial capital, and working-class sectarianism. Relying heavily on the bigotry and the "muscle" of the Orange order, the leaders of Ulster Unionism played on Protestant working-class fears of an insurrection by the supposedly disloyal elements in Catholic working-class communities.⁸⁹ The

⁸⁶ As Barbara Solow has written, "In Ireland in the 1880's, the landlord class bore the brunt of the blows. Rents, traditionally lagging and sticky, were rapidly readjusted to the falling price level under the influence of the Land Acts. The landlords were left to be squeezed between their inflexible costs and their declining rent rolls." *The Land Question and the Irish Economy*, 181.

⁸⁷ See the debates in the House of Lords in 1899 and 1900, when such Representative Peers of Ireland as Lords Inchiquin, Clonbrock, Farnham, and Templetown made impassioned pleas for state loans and other benefits as compensation for some twenty years of "confiscation" of landlords' rights. *Hansard, Parliamentary Debates*, 4th ser., 74 (July 18, 1899): 1128-63, and 85 (July 6, 1900): 728-60.

⁸⁸ For passing allusions to the role of landlords in organizing Unionist resistance to Home Rule, see Fred H. Crawford, *Guns for Ulster* (Belfast, 1947), 10-19; H. S. Morrison, *Modern Ulster* (London, 1920), 158-68; A. T. Q. Stewart, *The Ulster Crisis* (London, 1967), 32, 37, 52, 65, 69-73, 93-95; and Patrick Buckland, *Irish Unionism: Two, Ulster Unionism and the Origins of Northern Ireland* (Dublin, 1973), 21, 51-53, 58-61, 86. The relationship between "old" landed wealth and "new" business money in shaping and financing Ulster Unionism has been neglected by historians and deserves much deeper study.

⁸⁹ For the case for the (imperial) exploitation of (colonial) sectarian and class animosities, see Liam de Paor, *Divided Ulster* (Harmondsworth, 1970). For a decidedly more structuralist and controversial account of the "hegemonic" nature of Ulster Unionism (in which the landlords played no serious role), see Peter Gibbon, *The Origins of Ulster Unionism* (Manchester, Eng., 1975).

solvency of Ulster's larger landlords and the strength of the sectarian context in which they operated provide some explanations as to why Unionism proved an irresistible force in Ulster. In political terms the real difference between "northern" and "southern" landlords was that the former managed to endure well into the twentieth century as a class, sharing power and to some extent property through investments with the moneyed or business elite, while clinging to the high status of the old Protestant Ascendancy. By contrast, even the relatively solvent landlords (or landowners after the compulsory Land Purchase Act of 1923) who stayed on in the "south" after World War I survived only as individuals or families. They could still savor privileges denied to the mass of the population, but the former landlord element in the Protestant Anglo-Irish minority grew ever more isolated from the main currents of Irish life that pulsed just beyond the demesne wall.

In the long run, the economically depressed and politically volatile conditions of the late 1870s and 1880s inflicted far more damage on landlords than tenants. Incumbered owners who had been diverting only 20 to 30 percent of their income to interest payments suddenly found themselves paying upwards of 40 to 50 percent of their reduced receipts to meet these same obligations. The combination of heavy incumbrances and declining incomes spurred landlords to sell most of their estates after 1903. By selling holdings or farms to the occupying tenants through the agency of the Land Commission these landlords extricated themselves from a hopeless financial bind with some honor and, perhaps, a little cash in hand, once all the claimants had been paid off.⁹⁰

This analysis of Irish landed indebtedness has deliberately stressed the liabilities and neglected the assets of the landlords. It would be absurd to imply, let alone argue, that Irish landlords were paupers in princely guise or that all of their mortgages, bonds, annuities, and jointures prevented them from enjoying the traditional preoccupations of that class—Big House partying, shooting, pursuing foxes and heiresses, drinking and gossiping in gentlemen's clubs, competing with the local priest for the minds (if not the hearts) of the tenantry, improving the pleasure grounds, and subscribing money to anti-Parnellite associations. In the eyes of their tenants and laborers, these landlords were so rich that they could well afford the abatements of rent that were first requested and then demanded after 1878. But the combination of heavy incumbrances inherited from the past and economic adversity in the latter 1870s hastened the fall of a class whose capacity to govern had been steadily declining—at least outside Ulster—since well before the Famine.

⁹⁰ The 12 Percent Bonus on the purchase price, provided under the Wyndham Act, could be spent by the owner of a settled estate as he wished, while his trustees took over the residue remaining after all claims on the estate had been settled.

Reviews of Books

GENERAL

R. F. ATKINSON. *Knowledge and Explanation in History: An Introduction to the Philosophy of History*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. x, 229. Cloth \$14.95, paper \$6.95.

R. F. Atkinson's book introduces the reader to philosophy of history as it is currently understood. It does this job well, concisely, and in a reasonably sophisticated way.

Chapter 1 is rather traditional, setting up the distinction of analytic from substantive philosophy of history and identifying the two relevant senses of the word "history." The chapter's third section traces the subject from roughly the time of Oakeshott, Croce, and Collingwood (the 1930s) to von Wright (1971); this history is very capsulized. The section also offers a useful discussion of historians on the topic of philosophy of history, again very compressed.

Chapter 2 ("Knowledge of the Past") tells us that historical statements are not based on direct observation or, for the most part, on memory. Even testimony (from the past) requires special treatment to count as historical knowledge. What one would like then, but does not adequately get from the book, is a positive theory of evidential knowledge. This chapter also discusses, without resolving them, some of the interesting skeptical questions raised respecting historical knowledge. (I mean such questions as whether statements about the past are meaningful, and if so, whether we can know about the past evidentially, and whether there really was/is a past for such statements to fix on.) But that its pages should fail to "dry up the swamps of skepticism" (Danto's phrase) is, perhaps, not an important lapse in an introductory text.

Chapter 3 (on objectivity) is quite a good one and discusses a topic of real interest to the working historian. Chapter 4 (on explanation) sets out from the convention, established by Walsh and repeated by Dray, of dividing the field of explanation theory between the positivists (or "covering law" people) and

the "idealists" (whom Atkinson more properly calls the proponents of "rational" explanation). The important feature of this chapter, though, is the fact that the author introduces a third major theory of historical explanation: that such explanation is sui generis insofar as it consists in coherent narrative. Or, to put Atkinson's thesis differently, narrative history (under which he includes both narrative, in the conventional sense, and analytical treatments) is per se explanatory (see pp. 136-38 for a summary of his basic approach).

Atkinson's argument in chapter 4 rests, I believe, on a confusion. He shows, correctly, that the *meaning* of "explanation" is such that ordinary historical narratives must be regarded as explanatory. Moreover, he argues that neither of the standard theories—neither law explanation nor rational explanation—can capture all that is included under explanation through historical narrative. It does not follow from this, however, that ordinary historical narrative constitutes an account or theory of explanation. Atkinson has confused two distinct issues: which kinds of things are explanatory (as being in accordance with the *meaning* of "explanation") and which things are theories of explanation. Hence, historical narrative does not, simply in being explanatory, offer a theory of explanation at all, let alone an alternative to the standard ones.

Chapter 5 (on causation) follows lines laid down by White, Gardiner, and Collingwood. Atkinson stresses (p. 162) the openness and variability of historians' causal judgments. The chapter also includes interesting discussions of methodological individualism and of Marxism. The last chapter (on values) is well done. I particularly liked section 3 on moral judgments in history. I also thought section 4 on progress followed well. Since the two sections here are of central concern to historians and their students, it is fortunate they are good ones.

In sum, I think the book is well written and well organized. It compares favorably with the two principal introductory texts (by Walsh and by Dray) currently available. One of the strengths of Atkin-

son's book lies in its chapters, not found in the other two, on statements about the past and on values. On points where the books are comparable Atkinson's book holds up well, mainly because it is more complete (in the survey sense) and much more up to date. Atkinson's book does not include material on speculative philosophy of history. But inexpensive books of readings from the standard classics or good studies in paperback of such philosophies could be used to supplement Atkinson's book in a course.

Accordingly, I would stress the considerable intrinsic interest of the Atkinson book and its usefulness as a textbook. It should have an important place in philosophy of history.

REX MARTIN
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ANTHONY D. S. SMITH. *Nationalism in the Twentieth Century*. New York: New York University Press. 1979. Pp. x, 257. Cloth \$17.50, paper \$8.95.

Anthony D. S. Smith is emerging as one of the most important investigators of nationalism since Karl Deutsch. In *Theories of Nationalism*, Smith elaborated a sophisticated typology but fell short in his effort to establish nationalism as an ideology in which value is transferred from religious tradition to the community. In this, his second major book, Smith uses some of the best ideas of *Theories of Nationalism*, including the idea that nationalism is an ideology of rejuvenation, to analyze nationalism in the twentieth century.

The first few chapters are somewhat halting. Smith makes the sound but unexceptional points that nationalism is not really a millennial movement, that it must be clearly distinguished from fascism, which was not an extension of nationalism but a special European response to interwar problems, and that it should not be confused with racism. In these chapters he exhibits the same clarity of definition that characterized *Theories of Nationalism* but leaves the impression that the total result will be flawed, as was his first work.

In chapter 5, however, "Communist Nationalisms," Smith hits his stride. He suggests that Marxism and nationalism have conceptual meeting points that make it possible for them to coexist despite their overt antagonisms. The mating of the two ideologies occurs in the peripheral regions of the world capitalist economy, where delayed modernization and disappointment in the results of democracy force them together. The intelligentsia in these areas discover that Leninist party organization can provide political tools for modernization and that populist nationalism can mobilize the forces of ethnic or racial community to legitimate

the process. Their purpose is not social revolution, as their Marxist coloration might suggest, but the construction of political order, as Huntington called it, so that their nation can take an honored place in the international system of states.

Smith believes that nationalism will continue to prosper for at least two reasons. On the international level, the existence of a functioning worldwide system of states exerts enormous pressure for conformity to the ideal of the nation-state. On the local level, what Smith calls the bureaucratic cycle will continue to throw up ethnic nationalisms even within what seem to be well-integrated states. This cycle begins with the struggle for independence and the original organization of the nation. The next step is centralization of the state by the victors, which in turn generates revolt against the new center by an ignored periphery. At both the international and national level, the inhabitants of politically weak regions find it possible to make ethnic differentiations, sometimes very finely drawn, between themselves and the strong. The latent cultural affinities thus uncovered authenticate the political movements that seek to overcome the weakness.

Smith is a bit old-fashioned. He does not appear to be a Marxist, a structuralist, a model-builder, or a data-cruncher. His interpretation remains political. Nonetheless (should I say "therefore"?), Smith provides some satisfying explanations of why nationalism is still with us and why it probably will remain with us for a long time.

GALE STOKES
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NIKOLAI V. SIVACHEV and NIKOLAI N. YAKOVLEV. *Russia and the United States*. Translated by OLGA ADLER TITELBAUM. (The United States in the World: Foreign Perspectives.) Chicago: University of Chicago Press. 1979. Pp. xv, 301. \$12.95.

Rightly advertised as "a different view" of Russian-American relations, this book by two Soviet historians conveys what the Soviet government would like Americans to think the Russians think of those relations. For this reason, it differs from the other books in the distinguished University of Chicago series, which attempts to present genuine foreign perspectives on America's relations with particular countries. Moreover, the new Soviet venture in diplomatic history also differs from most previous ones by serving unabashedly the cause of detente as Moscow understands it.

Laudable as that cause may be, it does not necessarily provide the correct inspiration for good history. To be sure, compared with the thesis-infested and cantankerous Soviet historiography of the past,

Nikolai V. Sivachev and Nikolai N. Yakovlev's opus is almost a model of restrained factual analysis. But still, the thesis comes first, and the facts are harnessed to fit into it.

According to the thesis, Soviet-American collaboration is possible and inevitable for "objective" reasons, something that the Soviet leadership, privy to the wisdom of Marxism-Leninism, has always grasped perfectly but that less enlightened capitalist statesmen have frequently ignored to mutual disadvantage. Though sometimes compelled by the force of circumstances to promote better relations despite their worse selves, American leaders have all too often missed real opportunities for collaboration. Given these premises, the story of Soviet-American relations that unfolds in the book is fairly predictable, though not necessarily convincing for those not already convinced.

Thus, for example, the reader's credulity is stretched paper-thin by the authors' claim that the Soviet government bore no responsibility for the activities of the Comintern or by the assertion that Stalin's famous "election" speech of 1946 (cited in a suitably doctored version) was really quite conciliatory. Juxtaposed are examples of alleged American perfidy recorded, it is true, with more sadness than indignation. We are distressed to learn that the U.S. ultimatum to Japan in November 1941 was actually calculated to unleash a Japanese attack on the Soviet Union. In an effort to beat the dead horse of the second front, Elliott Roosevelt (of all people) is summoned as the crown witness to testify about Washington's putative scheme to delay the crucial operation in order to benefit from everyone else's exhaustion. When the Normandy invasion at last materialized in 1944, the hidden motive was presumably to counteract the triumphant spread of Soviet influence in Europe.

Besides the standard myths, clad in more respectable garb, there are some real surprises in the book, none greater than the authors' disposition to go out of their way to whitewash the tsarist autocracy for the sake of detente. Harsh words are reserved for the elder George Kennan and other, supposedly Jewish-manipulated, American critics of tsarist repression before World War I. This stunning condemnation of the nineteenth-century "slogan of Russian despotism as a hindrance to good Russian-American relations" (p. 15) speaks eloquently about the sensitivities and self-image of the Soviet Union's present rulers, for whom "the value of detente . . . lies in both nations adhering strictly to the principle of noninterference in each other's affairs" (p. 257).

On balance, the book is as good and as bad as the "peaceful coexistence" it is intended to buttress—a coexistence conceived as a "special form of the class struggle" (p. 40). In this respect, the study does not offer anything that American leaders or the public

do not already know or surely ought to know. Nor does it substantially enhance the horizons of professional historians by its selective bringing together of material familiar to them from elsewhere. For a book so redundant, prospective readers should think twice before paying even the very reasonable price for which it sells.

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WOLFRAM FISCHER. *Die Weltwirtschaft im 20. Jahrhundert*. (Kleine Vandenhoeck-Reihe, number 1450). Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. 110. DM 9.80.

Wolfram Fischer, the distinguished economic historian of the Free University of Berlin, has sought to compress the history of the world economy in the twentieth century into approximately seventy pages of text, eleven pages of endnotes, eighteen pages of tables, and five pages of bibliography. The book is an enlargement of a lecture given in Hamburg in October 1978, and the author regrets that he saw the fat volume of W. W. Rostow, *The World Economy: History and Prospects* (1978), too late to take it into account.

Three main parts consist of the structure of the world economy, trends and cycles in world economic development, and the developing countries in the world economy. The part on structure is divided into trade, terms of trade, production, services, income distribution, and summary. The ten pages on cycles cover the popular Kondratieff, recently adopted by Rostow, Forrester, and many others, the question of whether long swings have political causes, and imperialist exploitation of the Third World. The longer passage on the developing countries raises questions of agriculture, industrialization, export-led growth, and import substitution. The focus is evidently on developing countries in parts 1 and 2, as well as explicitly in part 3.

It is difficult to know what to make of this. Fischer is an able historian and economist, and what he covers he does well, if tersely. Where, however, are the themes of the decline of Britain as the dominant world power; the takeover by the United States after a dangerous interregnum that helped to produce or at least to intensify the world depression; World Wars I and II with their economic causes, if any, and their (different) economic aftermaths; the contrast between the tangle of war debts, reparations, and currency disarray in the 1920s and lend-lease, the Marshall Plan, and Bretton Woods in the 1950s? And so on. The data in the tables are fragmentary and overemphasize primary products. Chrome, bauxite, manganese, and molybdenum are

worthy products, but why no steel, automobiles, employment in engineering industries, electricity production? Many of the tables begin only in the 1930s, because they rely on the Woytinskys, who did not try to cover the whole century.

Perhaps the difficulty lies in the overambitious title.

C. P. KINDLEBERGER

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PETER KARSTEN. *Patriot-Heroes in England and America: Political Symbolism and Changing Values over Three Centuries*. Madison: University of Wisconsin Press. 1978. Pp. ix, 257. \$25.00.

Patriotism in the United States was one of the casualties of the Vietnam War. Today, diminished respect for authority and spreading cynicism, together with a decline in historical consciousness, make it difficult to engender enthusiasm for the great men of American history. It would seem that hero worship has become a thing of the past.

Peter Karsten does not choose to deal with the contemporary phenomenon. Rather, he is concerned with a time in American and English history when patriot veneration was a serious matter, especially for the elites who obtained political mileage from the practice. These elites made use of the symbolism associated with the names of various patriots to influence public opinion in ways considered desirable by them. It is Karsten's thesis that specific kinds of heroes were brought forward at different historical periods depending on the needs of the time. Later, they were discarded when their particular symbolism no longer served elite purposes. Thus, in a certain sense, each generation was made to celebrate distinct kinds of patriot-heroes.

When the American colonists rebelled against British tyranny, they cited the precedent of individual English heroes, like Algernon Sydney and John Hampden, who themselves had resisted tyranny. Decades later Americans used the memory of one of their own, Thomas Jefferson, to oppose the growing centralization of government. But a century later, the names of these three had all but been forgotten as the requirements of a new era called for different kinds of patriot-heroes. In an age of increasing governmental control and overseas expansion, figures who typify power and authority now came to the fore. Strong leaders of past ages, like Oliver Cromwell in England and Abraham Lincoln in the U.S., achieved first rank. The book concludes with the observation that the choice of patriot symbols reflects shifting values in political culture.

Karsten is probably correct when he argues that elite needs at different times help to determine which heroes get revered, and the book is full of ex-

amples and citations to bolster his views. But the kinds of general categories that he emphasizes, statism and antistatism, hardly exhaust the possibilities for the utilization of hero symbolism. Elites always seek to build up national fervor and loyalty, and this would be especially true in a country whose immigrant population needed to be Americanized. For this purpose, heroes like George Washington come to be bestowed with timeless virtues.

Moreover, the book occasionally falters when the author conjectures about the motivation or goals of ruling groups who, incidentally, are never identified. Frequently, the reasons Karsten gives for certain fashions in hero worship tend to be speculative ones. It can be demonstrated that particular names appear more often in contemporary texts at specified points in history, but establishing why this occurs is an entirely different matter and very hard to prove. *Patriot-Heroes in England and America* has raised some interesting questions about how ideas reflect changing social conditions, but it has not sufficiently answered them.

LAWRENCE KAPLAN

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ANCIENT

N. G. L. HAMMOND and G. T. GRIFFITH. *A History of Macedonia*. Volume 2, 550-336 B.C. Oxford: Clarendon Press. 1979. Pp. xxii, 755. \$65.00.

This book is the second volume of N. G. L. Hammond's projected three-volume series on the history of Macedonia. The present work covers the period from 550 B.C. to the death of Philip II in 336 B.C. and is divided into two parts. The first part is from the pen of N. G. L. Hammond and concerns the period of Macedonian history from 550 B.C. to the ascension of Philip II to the throne in 359 B.C. The historical sources for this epoch are notoriously scattered among various ancient writers, epigraphical evidence, archeological finds, and numismatic works. It is one of the important contributions of the present volume that all this material is conveniently assembled to elucidate historical events, movements of peoples, and the influence of Greek culture on Macedon. Interpretation of such unsatisfactory evidence is often difficult, and not all will agree with some of Hammond's conclusions. He accepts the Argive origin of the Macedonian royal dynasty, for example, rejecting the idea that it was a later invention to gain respectability in the Greek world. Hammond also advances strong arguments to support his notion that the term Argead refers not to the royal family but to a Macedonian tribe or clan. The narrative is illuminated by an excellent

color map (the lack of which was a shortcoming in volume 1).

The second and more extensive part of the book was written by G. T. Griffith and concerns the reign of Philip II. As Griffith observes, the major problem confronting historians is: how did Macedonia, heretofore a weak, divided state of negligible political and military importance, become during Philip's reign the ruler of the Greek world? The answer for Griffith and a great many others is that Philip himself was largely responsible for this achievement. Consider the situation in Macedon at Philip's ascension in 359 B.C. The former king Perdiccas had just been killed and the Macedonian army annihilated in a catastrophic battle against the Illyrians who were now preparing a major invasion of the country. Other neighboring Balkan nations were pillaging Macedonian territory. At home, Philip faced no fewer than five would-be usurpers, some supported by powerful states such as Thrace and Athens, which controlled much of the Macedonian seacoast. Griffith meticulously documents how Philip, by a shrewd combination of diplomacy, military intervention, and bribery gradually transformed his nation into the most formidable power in the Western world. Few will quarrel with Griffith's contention that Philip was instrumental in this transformation.

Of course, there is much controversial material here, and not everyone will accept all of Griffith's conclusions, especially concerning Philip's assassination, admittedly a frequently debated topic. Griffith accepts Aristotle's statement that Philip was killed for the private revenge of the assassin Pausanias, because Aristotle (who had close connections with Philip and Alexander) would never write such a statement if it was not generally known to be true. But how much knowledge did anyone really have of Pausanias's motives since he was killed within minutes after the assassination? Also, although there is a detailed chronology of Philip's plans to invade Persia, we are left quite in the dark about the reasons for Philip's decision. What events in Persia contributed to the decision to intervene?

But these are trifles. We are all in the authors' debt for the skillful and comprehensive way they have elucidated such a difficult period of Macedonian history.

DONALD ENGELS
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HEINZ KREISSIG. *Wirtschaft und Gesellschaft im Seleukidenreich: Die Eigentums- und die Abhängigkeitsverhältnisse*. (Schriften zur Geschichte und Kultur der Antike, number 16.) Berlin: Akademie. 1978. Pp. 133.

Heinz Kreissig's little book is an essay in structuralist interpretation; it aims to define the forms of ownership of the means of production and the social and juristic status of the producing classes in the Seleucid kingdom. Mostly this means agriculture, but Kreissig treats crafts and trade as well. The conclusions may be summarized: the Seleucids found a state in which "ancient Oriental" patterns predominated, did not aim to alter the situation, and in fact left things much as they found them. Farmers were mostly free but tied to the royal land they worked; slavery was almost unknown in production. The Greek cities founded or developed in the area had only a modest impact in introducing the private ownership of land and use of slavery characteristic of the polis. Kreissig is a minimizer, who sets himself against the more sweeping interpretations of his predecessors. The corrective is valuable, and Kreissig's overall judgment is probably not far from correct.

All the same, the book is not very satisfactory. Many pages are occupied in repeated discussions of the same handful of inscriptions and literary passages, broken up by the artificial organization of the book, and these discussions have little new to add. The inscriptions in Welles's *Royal Correspondence* are the most important evidence, and little is added to Welles's thorough and intelligent commentary, partly because Kreissig shows none of the needed philological acuity (the widest divergence, page 85 on RC 3, is simply wrong). The remarks on the *technitai* of Dionysus (p. 81) are an irrelevant howler. Bibliography is sketchy and references not always up-to-date: Dura parchments, for example, are cited by the first edition (1926); the pertinent volume of the Dura final report (1959) is listed in the bibliography but ignored throughout the book (to its detriment).

The decision to treat the Seleucid empire separately was unwise. The evidence is insufficient and scattered—too much can be and is made of many items—and almost all long known; Kreissig repeatedly must adduce evidence from other monarchies (Antigonos, the Attalids, the Maccabees) to flesh out the picture. Better to have taken all Asia (i.e., the former Persian Empire) for the entire Hellenistic period. Kreissig ignores Ptolemaic Egypt—the one kingdom about which we know something substantial—altogether, except for borrowing its terminology for land tenure (which he admits is mostly not attested in the Seleucid kingdom). Only token notice is taken of archeological material, especially in the section on settlement patterns (pp. 17–31).

A rewriting of Rostovtzeff has—wisely—not been attempted, but what we have is too little and too much. An article would have sufficed to set out the gist of the argument and its slight novelty. Eco-

nomic historians and those interested in the Hellenistic East will want to read this essay for its points of interest; but the student or scholar wanting to learn about the subject will do better to read Rostovtzeff (*Social and Economic History of the Hellenistic World*) and Welles's *Royal Correspondence* for synthesis and evidence.

ROGER S. BAGNALL
Columbia University

ION HORATIU CRIȘAN. *Burebista and His Time*. Rev. ed. (Bibliotheca Historica Romaniae Monographs, number 20.) Bucharest: Editura Academiei Republicii Socialiste România. 1978. Pp. 252.

This abridged version of the second Rumanian edition, translated by S. Mihailescu, details the life of the great Dacian chieftain Burebista, who was an ally of Pompey the Great and enemy of Julius Caesar. Social reformer, military strategist, and conqueror, he held sway from ca. 82 to ca. 44 B.C. over the northeastern areas of the Roman "barbaricum" from the river Tisza (Theiss) to the Black Sea, an area somewhat larger than that of Greater Rumania.

In the introductory chapters the author surveys the development of an autochthonous Geto-Dacian culture in Transylvania that he considers to be the foundation for Burebista's power. He shows how Dacia, in the crossroads between East and South, could resist for centuries turbulent tribal movements and was able to rise against Roman threats under Burebista, "the first and greatest of Thracian kings" (Syll.I.G.³ 762). Relying on available literary evidence (a brief account by a contemporary of Burebista, Strabo the geographer, and four additional, indirect references), the author discusses Burebista's life and his internal and external policy in three chapters. Three concluding chapters deal with the territorial limits of the area under Burebista's control, fifty-eight fortified settlements and fortresses within this area, and the culture of their peoples. The author assumes that Burebista was assassinated by disgruntled aristocrats shortly after the assassination of Julius Caesar.

The book is historical detective work *par excellence*. The few data about Burebista's life are treated with expertise. Specialists in the field should find valuable the discussions of Burebista's alliance with Pompey (pp. 46 ff.) and his campaigns (chap. 5). Regretfully, exciting new archeological evidence set forth in the book is not focused on Burebista's age. Pottery, small finds, and architectural data fall into a broad chronological span between the fifth century B.C. and the third century A.D. They are indirect and less than satisfactory allusions to the

much disputed hypothesis of Daco-Roman continuity (R. W. Seton-Watson, *A History of Roumanians* [1934], pp. 9-13). Consequently, many of the author's conclusions remain hypothetical and, occasionally, tendentious. Neither Dacian cultural originality beyond the natural borders of Transylvania, nor the existence of a Geto-Dacian state above an advanced tribal order appear to be proven by the evidence proffered. An index would have been a useful addition. For non-Rumanian readers, special problems are caused by the lack of a detailed map with generally known toponymy.

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ALDEN A. MOSSHAMMER. *The Chronicle of Eusebius and Greek Chronographic Tradition*. Lewisburg, Pa.: Bucknell University Press. 1979. Pp. 366. \$24.50.

Alden A. Mosshammer argues that Eusebius' *Chronicle* is important as "the earliest work extant in anything like its original form that deals with early Greek chronology in a continuous and comprehensive manner" (p. 16). Largely complete before 303, the work was half "raw materials" (much is lost), half synchronized lists. Neither part survives except in translation or re-edition, but enough remains so that by peeling off Jerome's additions one can get close to the original. In spite of Eusebius's Christianity he gives twice as much information for pre-Christian times as for post.

In this study we learn of Jerome's arrangement in columns, Greek chronography in general, Eusebius in particular. Mosshammer denies that the earlier Christian Africanus gave Eusebius an Olympic victor list. Indeed, there is little in the *Chronicle* from Africanus anyway.

After analyzing Eusebius' sources, Mosshammer turns to the dates assigned to various personages in early Greek history from Lycurgus to Euripides. In each discussion he examines Eusebius' date(s) and possible sources, their contaminations, and his or their errors. The upshot is likely to shake faith in Eusebius's skill and judgment, in spite of the author's soothing remarks. Take Hesiod. One date, 1017, is due to Africanus, who synchronized with Solomon; Eusebius further synchronized with the thousandth year after Abraham. The date 913 from *Latina historia* obviously comes from Jerome. This leaves 809, from Porphyry (who set Homer in 909, Hesiod a century later), and 767. Mosshammer shows that the last date was based on Sosibius's 867 for Homer, to which Eusebius himself added Porphyry's century. Another case: Thales. Here both Eusebius and Mosshammer seem on firmer ground,

though the extremely peculiar 747, found in the *Chronicle* with dates a century and a half later, seems neglected. (For 747, see Diels-Kranz *Vorsokratiker* 11 A 2 and 8.)

One can agree in principle with Mosshammer's conclusion ("every detail is suspect, but the traditional chronology is structurally sound" [p. 319]) while wondering how much help this gives historians working in areas supposedly served by the *Chronicle*. If every item has to be tested (as it does), what good is the whole work except as a sample of fourth-century encyclopedic erudition?

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MEDIEVAL

EDITH ENNEN. *The Medieval Town*. Translated by NATALIE FRYDE. (Europe in the Middle Ages, Selected Studies, number 15.) New York: North-Holland. 1979. Pp. 287. \$41.50.

This excellent survey of a vast and complex topic is doubly valuable. A comprehensive introduction to medieval urban history that will be of great value in undergraduate courses, it also introduces and comments on much of the field's extensive secondary literature. The thousand titles (plus a supplement) in Edith Ennen's bibliography represent a masterful work of selection, a list fifty-seven pages long made lively by citations in the text. On a very wide range of topics her text and its notes provide the best possible starting point for further reading.

The introduction begins (and the following two chapters continue) the task of defining what a town is and surveys urban civilization in the Roman Empire, emphasizing the West. Chapter 1 discusses the early medieval survival of Roman towns, their topography, institutions, trade, industry, and culture. Here, as always, analysis and narrative rest on a wealth of detailed information presented with great ease. Chapter 2 is devoted to new beginnings, the routes and settlements of merchants in Northern Europe in the early Middle Ages. Merchants who settled around Roman cities are discussed, but the emphasis is on the vast areas of Northern and Central Europe that Rome never ruled. Chapter 3 surveys the emerging medieval town in the tenth to twelfth centuries, ranging from Italy and Spain to Germany and discussing terminology, trade, architecture, fortifications, and more. Organization and governance are discussed in chapter 4, from the constitution of Genoa and other Italian cities to the foundation of new towns in Central Europe and to problems of social composition. Chapter 5, "The Organization of Economic Life," is less wide-ranging than the title implies; it briefly surveys fairs, the

origins of the Hanse, guilds, coinage, and Italian commercial sophistication. The longest and richest chapter is the sixth, "The European Urban Landscape," which begins with Italy and its trade, colonization, political struggles, and the *contado*. After discussing southern French towns, the author turns northward, ranging from the institutions of Cologne and Ghent to the topography of Novgorod. The Hanseatic League and its members are one of several groups of cities discussed and differentiated. Population, trade, and social unrest are the major themes of the last chapter; mention is made of the Jews, the clergy, and education. The attentive student will finish this book with an excellent basic knowledge of medieval European cities and an orientation to further reading, for, like most surveys, this one does not survey everything equally; there is relatively little, for example, on France. Institutional, political, and economic aspects predominate, reflecting the scholarly tradition shown so well in the bibliography.

Some passages struck this reviewer as confusing, unclear, or inaccurate; in every case the fault was the translator's. The Frankish legal system became Franconian (p. 100), *Muntleute* were "literally, mouthpeople" (p. 109). Elsewhere, the German text's meaning was reversed (pp. 74, 111, 113, 199), and confused renderings went uncorrected (pp. 71, 103, 152). In general, the English text lacks the clarity and liveliness of the original. Even so, the work itself remains excellent, a most important—and unrivaled—introduction to its subject. Its wealth of material on German cities is an enormous asset since so little is available in English on medieval Germany. The book is a welcome replacement of Rörig and Pirenne; despite its price, it should be in every library.

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JOACHIM W. STIEBER. *Pope Eugenius IV, the Council of Basel, and the Secular and Ecclesiastical Authorities in the Empire: The Conflict over Supreme Authority and Power in the Church*. (Studies in the History of Christian Thought, number 13.) Leiden: E. J. Brill. 1978. Pp. xvi, 514. f 144.

In recent decades, the Council of Basel has been studied extensively as a topic in ecclesiastical history, more specifically as a problem in the constitutional and intellectual history of the Church. Recent literature on fifteenth-century German constitutional history is for the most part available only to German readers, and even recent German publication on the period is not copious. This study is a superb example of a fruitful venture that ties to-

gether these two subfields in history, subfields that have all too often suffered from compartmentalization. Joachim W. Stieber focuses on the papacy and on the Council of Basel not as autonomous structures but as political powers negotiating with the German emperor and princes in an attempt to win their support. By the same token, he reviews German constitutional history in order to understand the context in which the German leaders made their decisions regarding ecclesiastical questions. The task is a bold one and the results are commensurate with the author's ambition. The notes and bibliographical appendixes alone constitute fundamental research tools for students of the fifteenth century.

The theme of the study is Realpolitik. The analysis of the motivations of the principal protagonist in the conflict is a prime concern of the author, and, in his view, lofty motives are hard to find. Eugene IV appears as a precursor of the Renaissance papacy, primarily concerned with protecting his political, financial, and military position. Likewise, the Council of Basel as it broke with Eugene IV is pictured as a self-seeking body concerned not about Church reform but about the defense of its power and prestige. A council called to reform the Church became ensnared in the same pursuit of wealth and power that it had hoped to extirpate from the Church.

Finally, the German princes, including the prince-bishops, emerge as "dynastic landlords." By this term Stieber means to reject any suggestion that princely actions resulted from systematic or planned programs characteristic of more modern rulers. Eager to protect and expand their landed base of wealth, the German princes were guided by these pragmatic concerns when they came together to discuss their reaction to the Council of Basel. Annates and clerical livings rather than spiritual loyalties directed their decisions. Frederick III is seen as little more than first among equals in decisions regarding ecclesiastical affairs and, indeed, as less than equal in regard to such qualities as decisiveness and boldness.

There are varying degrees of persuasiveness in the author's pronouncements about motivations. For the most part motives are discussed in social and political, not personal, terms. Only occasionally are more personal motives discussed, for example, those arising from the timid and cautious personality of Frederick III. At times declarations about motivations are substantiated by solid evidence regarding the financial or political advantages to be gained from certain decisions (for example, pp. 172, 195-96, 220-21). In other instances, however, the documentation about motivations is less convincing. The author retreats from the declarative to the subjunctive, the link between evidence and assertion is

less clear, and in some cases comments about motivation are speculations out of a general context of policy unsupported by specific footnotes (for example, pp. 164, 167-68, 219, 311, 320-21).

This study is historical art on a grand scale. The canvas is large, the color is striking, and the figure is sketched with clarity. For the present at least, this study offers a definitive interpretation of the relation between the Church hierarchy and Germany in the mid-fifteenth century.

PAUL L. NYHUS
Bowdoin College

MICHAEL GOODICH. *The Unmentionable Vice: Homosexuality in the Later Medieval Period*. Santa Barbara, Calif.: ABC-Clio. 1979. Pp. xvii, 164. \$14.95.

When does a subject of obvious current interest excuse the neglect of the most elementary canons of historical scholarship? Michael Goodich's volume insistently raises this question. The canons he has neglected range from the simple to the complex: (1) he cites documents he has not read, or, if he has read them, then he has not understood them; (2) he omits the contexts that might aid in interpreting his evidence; (3) he has no idea what conclusions to draw from his evidence; (4) his generalizations are consistently anachronistic. For want of space, I will present only one example of each type. Others may be found on almost every page.

(1) "One poem by the great courtly poet Guillaume IX of Aquitaine speaks of his love for the girl Agnes and for a certain Arsene (sic), perhaps a boy" (p. 6). The reference is to *Companho, faray un vers covinen*, where N'Arsen is clearly a woman. Arsendis, a common feminine name in the region, has no masculine equivalent.

(2) In presenting material from inquisitorial proceedings in chapter 1 and from various legal collections in the remaining chapters, the author pays no attention to the literary history of such accusations or legal enactments. He is therefore unable even to ask why attention was paid to homosexuality in some contexts and not in others and why the attention took the form it did.

(3) For just this reason, he does not ask (nor would he be able to give a reasoned answer had he asked) whether the accusations of "sexual non-conformity" (his phrase) imply that those accused were homosexuals or whether the inclusion of homosexuality among other sexual wrongs (such as clerical concubinage) implies anything about its prevalence.

(4) Chapter 3 opens (p. 41) with the interesting argument that the Church attacked sodomy in order to maintain a birth rate sufficiently high to sup-

port the colonization movements of the twelfth century.

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E. L. G. STONES and GRANT G. SIMPSON, editors. *Edward I and the Throne of Scotland, 1290-1296: An Edition of the Record Sources for the Great Cause*. Volume 1, *Introduction*; volume 2, *Texts*. (Glasgow University Publications.) New York: Oxford University Press, for University of Glasgow. 1978. Pp. xxvi, 285; x, 439. £50.00 the set.

Some six months after the death of the "Maid of Norway," who was the heiress apparent to the Scottish throne, Edward I summoned an assembly of Scots and English, the latter mainly of his council, to meet with him to decide who was the rightful king of Scotland. These meetings began in May 1291 and took place along the Tweed, at first in the environs of Norham and then at Berwick. They lasted with several adjournments until November 1292, at which time Edward decided that John Balliol had the best right to the vacant throne. The sessions during these eighteen months have been considered as one legal and constitutional process, and historians have given it the name of the Great Cause.

The present volumes are the result of twenty years of planning and work to bring together the records that were created by that process. These records and their composition are exceedingly complex in themselves. The most important were compiled by, or under the direction of, John de Caen, a royal notary, during the progress of the case. From various notarial materials he constructed a great roll in two exemplars of the proceedings. Many years later, between 1315 and 1318, another notary, Andrew de Tange, working under royal orders, compiled another great roll in three exemplars, based upon Caen and additional records. Both have been published, Tange's roll by Prynne (1670) and Caen's roll by Rymer (1705). Neither Prynne nor Rymer offered criticism of the rolls, and the two sources have never been compared.

These two notarial records, together with the *Annales Regni Scotiae*, form the three major narrative sources edited by E. L. G. Stones and Grant G. Simpson, and to these three they have added non-notarial documents, French and Latin texts and memoranda, and the opinions of French lawyers on the Great Cause. They have prefaced their edition of these materials with a comprehensive study of the major questions affecting the records. Considering the immense complexities of the subject, no one should be surprised that certain chronological questions and problems of textual derivation re-

main unresolved. The editors deserve only praise for the care and labor they have lavished upon this project.

Although Stones and Simpson did not intend to study the judicial process itself, they make clear their opinion that the assembly called by Edward should be considered a parliament despite the demurrer of Richardson and Sayles, that the proceedings were not an arbitration but an adjudication, and that Edward could have approached the question only through the essentially feudal posture of asserting overlordship in such a court of judgment. They believe that the tendency by scholars to regard the objectivity of notarial documents as unimpeachable should be abandoned, for a notary who creates records at the order of a king will execute many of the king's wishes in his work. On the question of why there are no Scottish records of the Great Cause, the truth would seem to be that, since a king, and especially the English king, did not permit record making in any of his law courts by anyone but his own clerks, he would presumably feel the same way about such a special court as that of 1291-92.

FRANKLIN J. PEGUES
Ohio State University

MARTA CRISTIANI. *Dall'unanimitas all'universitas da Alcuino a Giovanni Eriugena: Lineamenti ideologici e terminologia politica della cultura del secolo IX*. (Istituto Storico Italiano per il Medio Evo, Studi Storici, numbers 100-02.) Rome: The Institute. 1978. Pp. 209. L. 12,000.

As usurpers who interrupted a hereditary line of kings, Carolingian rulers not surprisingly sought legitimacy and justification for their own rule in theories of kingship and society. These theories, propounded by Carolingian thinkers and expressed by their sovereigns in official acts, were hardly unsailable bulwarks of political control. For one, theories of state that appealed to the divine order of things as revealed in Scripture contradicted the observable fact that effective power rested on control of land and the loyalty of vassals. Furthermore, the concept of the king as a vicar of God entrusted with ministerial power enhanced the monarch while leaving him open to ecclesiastical supervision and correction. Finally, as the ninth century progressed, the concepts of unanimity and concord with which writers of Charlemagne's generation had expressed the sentiments of the new order gave way to internecine struggle and institutionalized disunity.

Marta Cristiani charts with sensitive readings of the sources the efforts of such thinkers as Alcuin, Jonas of Orleans, Agobard of Lyons, Hincmar of Reims, and John Scottus to bring ideological stabil-

ity to a world in continual flux. Her book is essentially an essay in the history of ideas that seeks to uncover the multiple meanings of law and right order in society according to successive generations of ninth-century thinkers. It is to her credit that Cristiani never loses sight of immediate political exigencies that conditioned theoretical constructs. She is also to be applauded for her continual awareness of fundamental ambiguities in Carolingian political theory.

The first Carolingian attempt to fashion an abstract notion of the state was expressed by the concept of *unanimitas* as the convergence of individual wills and their embodiment in the will of the sovereign. The proper ordering of Christian society, however, demanded that the will of the community and its leader coincide with that of God. Thus a tension was created between secular authority and ecclesiastical authority, specifically the bishops who offered themselves as the interpreters of the divine will. While kings grandly appropriated the concept of divine grace to refer to their own bestowals on their followers, Hincmar of Reims pointedly reminded one of them that the priest's power of consecration set him apart from secular authority. In addition to tracing efforts to define more clearly the role of the monarch and the bishop in leading society, Cristiani provides an important analysis of the scriptural texts that served as the bases for Carolingian political thought. Theory and practice are joined in a chapter devoted to Charles the Bald's efforts to integrate concepts of justice and right order with vassalage. Her discussion of the role of the mid-ninth-century predestination controversy is particularly original. Jean Devisse, the most recent student of the controversy, has labeled it a "quarrel over words." For Cristiani, the predestinarianism of Godescalc of Orbais amounted to much more than a suspect theological statement. In limiting Christian society and the Church to the elect, Godescalc called into question the theoretical foundations of both Church and empire. Hincmar and John Scottus, both inspired by the newly translated Pseudo-Dionysius, responded to Godescalc's assault on free will with the reply that salvific grace was a gift that man was free either to accept or to refuse. Christian society, furthermore, was a universal one whose laws embraced both the saved and the sinners, whom, it was hoped, would be called eventually to repentance through the mediation of the Church and its sacraments. In place of Godescalc's fractured view of society, Hincmar and especially John Scottus offered a unified, hierarchical structure. Cristiani argues that John Scottus further elaborated his theory of an organic society held together by immutable laws in his most important theoretical work, the *Periphyseon*—a work whose vision of society, because of the actual political cli-

mate, would remain incomprehensible to most contemporaries.

In the earlier portion of her book, Cristiani generously acknowledges the work of Beumann, Ganshof, Tabacco, Kantorowicz, and other scholars. (Curiously, the name of Karl Morrison is missing both from the footnotes and the bibliography.) In asking her readers to regard John Scottus, upon whose thought her own previous work has focused, as a politically inspired metaphysician, she makes her most interesting and stimulating contribution to the history of Carolingian political theory.

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JEAN LECLERCQ *Monks and Love in Twelfth-Century France: Psycho-Historical Essays*. Oxford: Clarendon Press. 1979. Pp. x, 146. \$19.95.

Although much attention has already been paid to the courtly love poetry of the twelfth century and a number of contending theories, some literary, others sociological, have been presented in order to explain this sudden efflorescence of erotic interest, very little interest has been shown in its contemporary ecclesiastical counterpart. Thus, Dom Jean Leclercq's collection of essays—which includes discussions of the exegetical treatment of the *Song of Songs*, the influence of Ovid in the twelfth century, the literary and sociological milieu of Champagne, and the underlying psychological trends in St. Bernard's career—is a welcome introduction to the psychohistory of the post-Gregorian age that makes use of previously unexplored monastic material. Some tentative psychohistorical conclusions are drawn, but with none of the partisan rancor (often based on insufficient evidence) that has occasionally marred other attempts to distinguish conscious and unconscious double meanings in medieval literature.

One could, however, argue with several of his basic assumptions. He speaks of three dimensions of human need, the physiological, psychosociological, and spiritual; some would argue whether the "spiritual" has an independent existence. Despite the patent sexual imagery found in the works of Jean of Fécamp, Anselm, and others, Leclercq suggests that divine, chaste love, unhampered by stirrings of the flesh, lay behind these musings (the *Song of Songs*, for example, was often regarded as a symbolic description of the relationship of the believer to the Church or of a cenobite to his monastery) and that St. Bernard and his contemporaries were not obsessed with sexuality. If, as Leclercq argues, the twelfth century was a "less erotically preoccupied society" than our own, what explains the great wave of canonical legislation dealing with marital

and sexual matters, the erotic nonconformity of the new heretical sects, the vogue in monastic flagellation, the rise in female vocations, and the spectacular royal divorces that characterize the period? And how should contemporary treatises on friendship be treated? As continuations of the Ciceronian literary tradition or as documents of sexual interest? And what emotional conflicts underlay the cult of the Virgin? As Leclercq's work has shown, seemingly drab monastic literary meanderings may often mask a society undergoing profound psychological change.

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ANTONIO GONZALEZ GOMEZ. *Moguer en la baja edad media (1248-1538)*. (Publicaciones.) Huelva: Instituto de Estudios Onubenses "Padre Marchena." Excma. Diputacion Provincial de Huelva. 1977. Pp. 301.

In recent years the departments of medieval history of the Universities of Seville and La Laguna (Tenerife) have encouraged the study of local and community history in late medieval Andalusia. Antonio Gonzalez Gomez's work on the town of Moguer, offered as a doctoral dissertation in 1974, is an excellent example of these efforts. It is particularly welcome since few detailed analyses of the institutions and populations of towns under seigneurial jurisdiction have been published. Moguer is a small community on the Río Tinto, a few miles from the city of Huelva. Taken from the Muslims in 1248, it was granted as a seigneurial holding in 1333 and remained in the hands of the Portocarrero and Tenorio families for the remainder of the medieval period.

The description of Moguer consists of six chapters of unequal length, commencing with a study of the genealogy and careers of the Tenorio and Portocarrero families. Because of the extent of the subject and the limited evidence available, the treatment is necessarily uneven, but it provides the justification for the claim that the work covers the period from 1248 to 1538. The description of Moguer is, in fact, restricted to the late fifteenth and early sixteenth centuries. A short chapter on geography serves to set the scene but fails to provide any of the analyses that a well-developed historical, cultural, or economic geographical approach might have made possible. Separate chapters treat the economic exploitation of the district—primarily agricultural in accordance with seigneurial domination but including some husbandry, fishing, and salt-working—and its land and water commerce. The most extensive chapter is that devoted to the discussion of the economic and social groups comprising the local population. The author does not dispose of suf-

ficient data to allow any solid statistical or demographic analysis but is able to reiterate the dominant role played by the primary sector, and agriculture in particular, in the local economy. The work concludes with a short description of the offices and functions of the municipal government, a short summary of conclusions, and an appendix of fourteen documents, dating from 1333 to 1519, important to the history of the district.

In and of itself, the book has many limitations. It provides a description of Moguer at the turn of the fourteenth century and conveys little of the development of the community over any extended period of time. By concentrating upon the district so exclusively, Gonzalez fails to develop fully the role of the regular English and Breton sea-trade using the port of Moguer, the effect of the far-flung commercial and corsair activity pursued by the Moguerenos themselves, or the influence of the cosmopolitan transient population of the port. There is far more data presented than evaluations or conclusions. Gonzalez's book should not be considered simply as an independent work, however, but within the context of the studies being carried forward by his school. A number of similar investigations of local communities will be necessary before the complexities of the era and region are unraveled and effective syntheses and interpretations can be formed. This study of Moguer is an effective step toward that goal.

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KARL JORDAN. *Heinrich der Löwe: Eine Biographie*. (Beck'sche Sonderausgaben.) Munich: Verlag C. H. Beck. 1979. Pp. xi, 316. DM 38.

While *kleindeutsch* historians perceived Henry the Lion as the first representative of a national German feeling and the proponent of eastward expansion, their *grossdeutsch* colleagues saw the Welf duke of Bavaria and Saxony as a rebel who betrayed Frederick Barbarossa on the eve of Legnano. Fundamental to both assessments was the belief that Henry and his cousin were antithetical figures. Such interpretations reached their climax in 1935 when the Nazis turned the church of St. Blaise in Brunswick, built by Henry and his burial place, into a national shrine. Thereafter, though, as Nazi foreign policy became more openly aggressive, it was the emperor who was hailed as the embodiment of Germany's imperial destiny. Karl Jordan, the author of several scholarly monographs about Henry, has written a popular and readable biography of the duke that is free of such nationalistic and anachronistic interpretations.

Jordan's Henry is a twelfth-century prince. The

old constitutional order of loosely organized tribal duchies, based on personal ties, had been destroyed during the investiture conflict and was being replaced by smaller territorial states. Since the Welfs possessed few allods in Bavaria, Henry focused his attention on Saxony, where, as the heir of several dynasties, he possessed extensive holdings as well as counties and advocacies. He tried to weld these together into a principality by a ruthless assertion of his rights and by further acquisitions. He was especially interested in Nordalbingia and Mecklenburg, which he conquered and opened to German settlement. Henry's fatal mistake was to forget that his success depended on his alliance with Frederick. When Henry refused to provide the emperor with military help at Chiavenna in 1176, Barbarossa became responsive to the complaints of the Saxon princes, whose own territorial ambitions had been thwarted by Henry. Henry's condemnation in 1180 destroyed the last vestiges of the Bavarian and Saxon tribal duchies and completed the transformation of Germany into a confederation of principalities organized along feudal lines.

The major shortcoming of this sensible book is suggested by its subtitle. The proprietary and legal orientation and the annalistic style of medieval sources are not conducive to the writing of biographies. Essentially, this is a traditional political history, in which Henry's actions have been placed in the broader context of imperial politics. There is, for example, no way of knowing Frederick's and Henry's personal feelings about each other before and after Chiavenna. Beyond that, Jordan pays little attention to the fundamental social and economic changes Germany was undergoing. The development of the territorial states depended on the extinction of most of the old German nobility, but Jordan provides no explanation of why this happened. Nevertheless, this book provides an excellent introduction to the most important German prince of the era.

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ILLUMINATO PERI. *Uomini, città e campagne in Sicilia dall'XI al XIII secolo.* (Collezione Storica.) Rome: Editori Laterza. 1978. Pp. 358. L. 12,000.

Since at least the *settecento* historians have observed the contrasts between northern and southern Italy and have debated to what extent the economic problems and poverty of the south can be explained in either geographical, deterministic terms or in terms of human decisions and cultural history. This is the essential historiographical concern underlying Illuminato Peri's book. His survey of medieval Sicily begins with and concentrates heavily upon Is-

lamic-Norman Sicily, a land with a reputation for fertility and wealth. His main theme, as he describes the port cities, the *casali* or agricultural settlements, the rivers, and the architectural styles, is that Sicilian prosperity in the Norman period rested upon a delicate equilibrium, one not only economic but cultural and political as well: between port cities and agricultural hinterland, Christians and Muslims, followers of Greek rites and Latin rites, and between the ruling dynasty and the conquered population. Moreover, Peri maintains that Norman Sicily's prosperity was not self-generating but that the *coniuntura* between economy and society depended upon immigration from continental Italy.

According to Peri the turning point for medieval Sicily and the beginning of its decline came under the Swabians. Behind the traditional Burckhardtian image of Sicily as the most politically progressive of Italian states in the thirteenth century, Peri finds conservative economic and fiscal policies, which, he claims, destroyed the fragile balance of Sicilian prosperity. Peri acknowledges the achievements—the legal compilations, the scientific treatises, the poetry—of the “functionaries” class under the Swabians, but clearly for him these contributions cannot compensate for Sicily's continued dependence upon an agricultural-pastoral economy and its failure to develop the cities, the merchants, and the artisans characteristic of central and northern Italy. For Peri the decline of medieval Sicily was not simply determined by geography but resulted from geographical limitations and the destructive policies of Henry VI and Frederick II. For example, the Swabians, in not recognizing the vital importance of immigration for Sicilian prosperity, pursued policies that reduced even further the dwindling flow of immigrants from the mainland. The regressive policies of the Swabians were continued and intensified by the Angevins; and, in addition, Sicily was swept by disasters such as the contracted navigability of rivers in the southeast, famines, epidemics, and wars. The Angevin government responded with policies calculated not to alleviate the conditions of alternating periods of abundance and famine but to profit fiscally from them.

Peri's interpretation is a deeply pessimistic one, and unfortunately this viewpoint leads him to see decline even when the evidence is tenuous or negative at best. For example, Peri finds “stagnation” in early thirteenth-century Sicilian agriculture, even though he acknowledges the relatively high yields of Sicilian grain in comparison with other regions. His evidence for agricultural “stasis” is his perception of a lack of technological innovation in thirteenth-century Sicilian agriculture; but, since he does not compare the level of Sicilian technology with that of other regions, his evidence has limited significance. The book has other weaknesses. There are

too may brief, discontinuous chapters, and too frequently one is buried under a tediously detailed treatment of an isolated document. In general, however, the book contributes a basic and needed revision of medieval Sicilian history from the viewpoint of the new *storia strutturale*.

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PETRE DIACONU. *Les Coumans au Bas-Danube aux XI^e et XII^e siècles*. Translated from Rumanian by RADU CREȚEANU. (Bibliotheca Historica Romaniae, Études, number 56.) Bucharest: Editura Academiei Republicii Socialiste România. 1978. Pp. 158. 8.25 L.

The Cumans, an important Turkic confederation of the trans-Danubian, Ukrainian, and western Central Asian steppelands in the two centuries preceding the Mongol invasions, remain one of the least studied groups of Eurasian nomads. Their role in the history of the Rus' principalities, Hungary, Bulgaria, Rumania, and Byzantium was profound and lasting. Regrettably, this slim volume adds very little to the literature on Cuman history. Petre Diaconu has very narrowly defined his topic, often forgetting that these nomads interacted with a number of peoples and territories simultaneously. Consequently, the larger perspective frequently fades from view.

Eschewing political history (for which he directs the reader to the old, but still useful studies of Rasovskii and Zlatarski), Diaconu promises a more detailed treatment of questions of historical geography, numismatics, and sigillography. Had this promise been fulfilled, the book might have been regarded as an important contribution. Unfortunately, it is a series of brief excursions summarizing previous research and occasionally buttressing it with addenda from recent Rumanian archeological investigations.

The Rumanian territories are extremely rich in Turkic (most probably Cuman) toponymy. This material is discussed in only one brief, and amazingly unsophisticated, chapter. Diaconu is apparently ignorant of the modes of toponymic analysis used by Fuad Köprülü to determine the settlement patterns of the Oghuz Turkic tribes in Anatolia or the more recent study of Hungarian tribal settlement patterns done by Györffy (*Archivum Eurasiae Medii Aevi*, 1 [1975]). He ignores, on the whole, the more than century-old tradition of Cuman studies in Hungary (for example, the recent work of Rásonyi, Pálóczi-Horváth, and Mándoky). The studies of Rumanian Turkologists specializing in Cuman philology (for example, Drimba) are also absent.

The occasionally proffered etymology of one or another alleged Cuman term or place name displays an almost saintly innocence of Turkic and related fields. This is very unfortunate, for a careful toponymic analysis might have revealed to the author the extent of the Cuman occupation of the eastern Rumanian territories.

Much of this study centers on the Byzantine sources for the Cuman Balkan raids of 1094, 1114, and 1122–23, some minor points of which are argued in great detail. Diaconu is probably right (pp. 67–70) in his identification of the “Scythians” of the 1122 events with the Cumans rather than with the Pechenegs, as has hitherto been generally accepted. His attempt to equate *Blökummannaland* of the *Heimskringla*, an Icelandic source for the 1122 raid, with *Vlachia* is typical, however, of his philological acumen. Diaconu makes a valiant effort to prove (without adducing much in the way of evidence, either literary or archeological) that Oltenia, Muntenia, and much of Moldavia were under either Cuman or Byzantine rule during this period. This allows him to challenge the claims of some Soviet historians that these or parts of these territories fell under the sway of the western Rus' (Halych) princes. Diaconu, unaware that we may now distinguish between a number of politicogeographical units within the Cuman union, makes no attempt to differentiate among the various Cumans involved in the activities of Ivan Rostislavich “Berladnik” in the “Danubian towns” or among the steppe allies of the Asenids of Bulgaria. This is a serious methodological flaw.

Perhaps the most important part of this work is the annex (pp. 134–38), which describes, with maps, the distribution of Byzantine coin finds on the lower Danube. In all other respects, this is a remarkably unsophisticated and superficial work.

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MODERN EUROPE

PETER BURKE, editor. *The New Cambridge Modern History*. Volume 13, *Companion Volume*. New York: Cambridge University Press. 1979. Pp. vi, 378. \$29.95.

The idea of adding a collection of thematic essays to a prestigious series that emphasizes narrative history is an imaginative one, and it has on the whole paid off. The essays deal with a number of major features of European history since 1500—some emphasize Western history, others cover Europe more generally—including warfare, bureaucracy, secularization, environment, peasantry, and demography. Inevitably, one wonders about the exclusion of certain

topics; the subject of urban social structures and mentalities is largely omitted, for example, as is any real effort to characterize European diplomacy. The volume leans toward intellectual history—indeed, a final essay on Western civilization for the past 2,500 years conventionally defines great ideas as the essence of Westernism, which I think overstates the case.

The inevitable unevenness of the essays should not detract from the utility of the volume. With very few exceptions (I found the essay on warfare simply too brief, that on social science a bit shallow), the essays are at least well-informed and thorough summaries, and at best they are really stimulating. My preferences included the essay on environment, necessarily somewhat tentative in periodization but at least serving as an early benchmark in the field; that on the scientific revolution, which, adopting a social context and relating seventeenth-century developments to an older intellectual framework, suggests a redating of what was really revolutionary about modern science; and Peter Burke's intelligent statement of the complexities of charting secularization.

The essays are not interconnected, and in certain cases some exchange would have facilitated not only coordination but accuracy. Thus, the final piece reflects no understanding of the kind of change in popular mentality that Burke and Le Roy Ladurie discuss. The timidity of the demographic essay is also striking: descriptive changes are duly noted but discussion of causation, which Americanists increasingly seek in aspects of *mentalités*, is sedulously avoided.

The volume as a whole was supposed to be informed with a consideration of whether the French and Industrial Revolutions constituted a key break in the phenomena being considered. The editor was properly concerned to use the essays to escape the focus on short-run change, inevitable in the preceding narrative volumes, and to deal with major change and the equally important theme of continuity. Some of the essays add little to fairly well-accepted periodization schemas; thus, Le Roy Ladurie accepts, perhaps a bit facilely, the idea of the late nineteenth century as a key break for the peasantry. On the whole, the essays seem more at home with early modern than with modern history, and few come to grips with the problem of deciding whether there has been major change *since* the Industrial Revolution. Few indeed try to place the twentieth century in any serious analytical context.

The final essay, more daring than the rest but also on balance less useful, does venture a bit of prediction, noting that we may be heading back toward a more medieval civilization in the pendular swing of Western culture from real Westernism (ancient, modern) to non-Western intrusions (Middle

Ages, the future). As is suggested by the inclusion of this rather playful avoidance of the very issue of modern periodization with which some of the other essays grapple, the volume comes to no overall conclusion, even about the criteria that should be used to measure major periodization. One remains with some very interesting individual essays, whose linkage, however, must be sought elsewhere.

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DOROTHY KOENIGSBERGER *Renaissance Man and Creative Thinking: A History of Concepts of Harmony, 1400–1700*. Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. xiii, 282. \$25.00.

Dorothy Koenigsberger attempts a fresh look at the Renaissance notion of harmony, studying it less as an explicit theory than as a series of related assumptions underlying the various facets of the period. To achieve her goal the author analyzes the works of Leon Battista Alberti, Leonardo da Vinci, and Nicholas of Cusa. After establishing the links among these writers, Koenigsberger then takes up the practice and theory of music in the Renaissance and concludes by discussing the Renaissance interest in interdisciplinary studies.

The thesis of the book is hard to state simply. The author argues that the notion of analogy is central to Renaissance harmony, that Renaissance analogy differs from medieval analogy in being less hierarchical, more grounded in actual experience, and more convinced of the natural sympathy among the parts of reality. Alongside this main thesis are several subsidiary ones. The author feels that Nicholas of Cusa exercised a decisive influence by framing artists' notions of Platonism in the early fifteenth century, that magic ran counter to the harmony her subjects sought, and that the universality implied by analogical thinking led to the interdisciplinary nature of Renaissance scholarship.

Several factors make the argument difficult to follow. The author too frequently concludes a detailed discussion of a text with a commonplace that adds little to our understanding, and she devotes far too much space to explaining what she is not saying. In addition, the book seems unnecessarily disorganized and hastily written. A chapter on Nicholas of Cusa is inserted without sufficient justification between two chapters on Leonardo. To make matters worse the first chapter on Leonardo refers to the second as if it were to follow immediately. Obviously the chapter on Cusa was inserted as an afterthought. Finally, the editor should have corrected the grammar of the book, which exhibits lack of parallel structure, dangling participles, and colloquialisms that distract the reader from the book's thesis.

Apart from these issues of organization, style, and grammar, there are some important conceptual problems. First, the basic contrast between Renaissance and medieval analogy rests on an overly simple view of medieval thought. The author usually refers to Boethius as her exemplar, although at one point she makes good use of Bonaventure. Dante, much more important to Renaissance thinkers themselves, is barely mentioned, and the author seems unaware of Mazzeo's excellent treatment of medieval analogy in Dante's work. Had Dante been taken into account, the picture of Renaissance analogy presented in the book would be far more sophisticated. Second, while it is refreshing to view Alberti and Leonardo outside the context of Florentine intellectual life, the author needs to consider more explicitly the importance of that tradition, to which only a brief section is devoted. Finally, the author, in trying to probe the unspoken assumptions that underlie theories, has ignored many important models. In particular her attempt to show the implicit contributions of the Platonic Academy to science could have benefited greatly from a reading of Thomas Kuhn.

This is an ambitious work on an important subject that unfortunately does not quite deliver what it promises.

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M. S. ANDERSON. *Historians and Eighteenth-Century Europe, 1715-1789*. Oxford: Clarendon Press. 1979. Pp. vi, 251. \$26.00.

Historians do not usually write book-length historiographical essays. M. S. Anderson's history of eighteenth-century bibliography will, therefore, prove to be an extremely useful teaching aid for graduate students and specialists alike. Basic trends in French, German, and British historiography are delineated, with the French and British ones receiving the most adequate treatment. The most attention is devoted to changing views of the French Revolution and the *ancien régime*, from the contemporary reaction to it to the modern *Annales* school. French historians have shifted their focus from studying the revolution in Paris to emphasizing regional history. The classical historians are also given an excellent treatment: Michelet, Taine, Tocqueville, and Jaurès in particular. Useful also is the picture of the rise of quantification studies and the importance of the work of Ernst Labrousse in this context. Anderson supports the French claim that the "vitality" of their social and economic history has no equal in the world today.

Narrative and diplomatic history is downgraded, although some rather obscure eighteenth-century

works are mentioned. The recent German emphasis on "modernization studies" (Koselleck, for example) is not mentioned, and the excellent German schools of regional history are completely omitted. Yet the short sketch of Frederick the Great's biographers and those of Joseph II is extremely well done. Less satisfactory for the practicing Germanist is the author's argument that the concept of enlightened despotism turns out to be meaningless. Certainly German regional history in this area is as rich as the French, though less well known in the English-speaking world. What was new and different about the theory and practice of government under enlightened despotism has been worked out by several contemporary North American historians whose work has not been included: H. Liebel, H. C. Johnson, F. Szabo, and especially Leonard Krieger of Chicago. The important work of Walther Hubatsch, who has produced an entire school of young German historians working in this era, is also ignored. Overdrawn is the author's picture of Frederick II and Joseph II as rulers who were merely continuing age-old policies. No one who is familiar with Continental archives would find such a view to be at all tenable. Once confronted with the mounds of documentary material dealing with the reforms and innovations effected by these rulers, one cannot believe that enlightened despotism is reducible to "a set of theories and aspirations."

Most interesting as well is the author's survey of the changing picture of George III. His contemporaries viewed him favorably, but the nineteenth-century Whigs attacked him for seeking to undermine the constitution. Lewis Namier's methodological revolution in this century then brought in the new emphasis on sociological analysis, which was applied to biography. The circle comes full, then, and the old interest in political history reappears in its new social guise.

HELEN P. LIEBEL
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LUCIANO ALLEGRA and ANGELO TORRE. *La nascita della storia sociale in Francia: Dalla Comune alle "Annales."* (Fondazione Luigi Einaudi, "Studi," number 22.) Turin: Fondazione Luigi Einaudi. 1977. Pp. 355. L. 7,000.

French scholars have recently complained that the *Annales* historians are much praised in Italy but not sufficiently imitated. Italian historians may have neglected to give the *Annales* this ultimate flattery, but they are paying critical attention to French historiography. Luciano Allegra and Angelo Torre's splendid account of the origins of French social history is the most important of current reflections on French historical discourse. Torre is responsible for

the first part, "Il superamento del positivismo," and Allegra for the second "Una metodologia per le *Annales*."

Allegra describes the essential characteristics of the *Annales* methodology as a commitment "to posing problems and formulating hypotheses" (p. 273). This is the path Torre and Allegra follow in tracing the development of French social history. Modern French history is seen as beginning with Numa-Denis Fustel de Coulange's *Histoire des institutions politiques de l'ancienne France* (1875). In that work, Torre observes, Fustel de Coulange asserted that institutions are always "the expression of the cohesion among customs, laws, material interests, the way of thinking and the 'spirit' of the generations that govern" (p. 31). The subsequent development of French social history did not follow a linear course from Fustel de Coulange's directions. Constitutional, political, and legal historians contested the validity of the more socially oriented history that was responding to the progress of socialism, sociology, and the materialist philosophy of history.

The fruitful dialogue between history and sociology in the *Année sociologique* further extended historians' appreciation of the nature of society, but it did not decide what labor was specific to history and what was particular to sociology. Henri Berr's *Revue de synthèse historique*, while encouraging regional history, convinced historians to incorporate in their work the new findings and concepts of geography, linguistics, ethnography, and studies of popular culture. Torre's analysis does not treat, however, only those who later became great stars in the French historical firmament; he concretely and informatively cites hundreds of historians who added incrementally to the growth of historical method and practice. Both authors repeatedly stress the chronic struggle waged against political and event-centered history. The evidence they collect suggests, however, that the traditional historians were overwhelmed by the new wave of social history already in motion before World War I.

The postwar years offered the opportunity to exploit and expand the theory, method, and example that would give social history its hegemony in France. Allegra explicates the secrets of this success in the second part of the book. The confrontation with communism, the impact of Maurice Halbwachs, and the decisive work of the *Revue d'histoire économique et sociale* are incisively examined. The *Annales*'s programmatic force, from its founding in 1929 until the present, was possible because of all that was accomplished in the previous fifty years of historical controversy and inquiry.

Today, the *Annales* is self-consciously experiencing earnest doubts about the unity of its purpose. This penetrating study, accenting the labyrinthian ways of historical discourse, illuminates and makes in-

telligible social history's complicated past, and it also makes explicit the generic problems likely to shape this history's present and future contradictions and traumas.

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CHRISTOPHER DUFFY. *Siege Warfare: The Fortress in the Early Modern World, 1495-1660*. Boston: Routledge and Kegan Paul. 1979. Pp. xii, 289. \$22.50.

Christopher Duffy is the author of a number of well-received volumes on the military history of the eighteenth century and the Napoleonic era—*The Wild Goose and the Eagle* (1964), *Borodino and the War of 1812* (1973), *The Army of Frederick the Great* (1974), *The Army of Maria Theresa* (1977), and *Austerlitz* (1977). All—institutional history, battle accounts, or biography—were marked by clear, lively writing, good use of sources, and excellent illustrative material, as well as a certain dry humor. Perhaps inevitable for a military historian of this period, in which warfare was so often conducted within the parameters of a fortress environment, Duffy also developed an interest in fortifications, and the result was *Fire and Stone: The Science of Fortress Warfare, 1660-1860* (1975).

The present volume is a companion book to *Fire and Stone* that examines the centuries during which fortress warfare gradually emerged in the forms that would prevail in Europe until the mid-nineteenth century. It displays all the characteristics of Duffy's previous books—extensive learning, clear style, well-chosen and well-executed illustrations, substantial documentation, and a judicious bibliography.

As his point of departure, the author has chosen 1494, when a French army under Charles VIII, equipped with a strong and effective artillery, burst into Italy and rapidly shattered its complex and delicately balanced political system. Castles and fortresses with which the Italian rulers previously had protected their possessions quickly were subdued by the new weapons. As Guicciardini, the great historian of this period, wrote, sieges that previously had taken months now took days or even hours. The events of 1494 forced a basic re-evaluation of fortification systems.

The Italian response was a new type of fortification to replace the medieval castle, which had repelled attack primarily by physical obstacles. The new model, based on a bastion trace subjecting the attacker to an effective cross fire, provided an answer by turning walls and ramparts from mere obstacles into fire bases. Concentrating his fire in turn, the attacker made progress in battering breaches

into the new ramparts, while the defense replied by adding outworks and arming fortresses with more far-reaching artillery. Italian, French, and Dutch schools of fortification and siegecraft developed, ultimately culminating in the work of the great Vauban, master of fortress design and siege warfare.

But this book is not concerned primarily with the technicalities of fortress design, armament, or even siege warfare, though it does cover these, too. It is a survey rather than a monograph, and its main purpose is to analyze warfare in a period and in an environment ever more dominated by sophisticated and costly fortifications. It embraces military as well as social and economic dimensions and does not restrict itself to Western Europe, but also deals with Eastern Europe, the Ottoman Empire, and the Far East, although here, especially in the dominion of the Turk, the art of fortification and siege warfare long remained rudimentary.

Not unlike logistics, a subject often neglected in conventional writings on campaigns, fortifications provided the basic framework for most of the wars during this period, dominating strategy and requiring, at least in Europe, the vast and cumbersome siege trains that slowed down campaigns and made logistics so all important. Beyond that, the development of siege warfare, as Duffy points out, required the centralization of resources and gave an impetus to the rise of the modern bureaucratic states. Scholars, buffs, and plain historians will find this an interesting and useful book.

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ERIC J. LEED. *No Man's Land: Combat & Identity in World War I*. New York: Cambridge University Press. 1979. Pp. xii, 257. \$14.95.

Most studies of World War I, like those of other wars, are "history from above" in treating causes, course, and consequences. What little we sense of the individual's experience derives largely from specific novels, poems, and memoirs rather than analytical "histories from below." Leed's work responds to this need and will be compared to Paul Fussell's *The Great War and Modern Memory* (1975), a comparison worthy of both authors and instructive to readers. Whereas Fussell focuses on literary and linguistic responses, Leed is concerned with psychological results. By reaching many of the same conclusions from different starting points, the two books validate and complement each other. Taken together, they deserve to be regarded as the seminal works on the individual's experience of war, its implications for Western society, and the nature of modernity.

Leed's main concern is the transformation of per-

sonality by war, in particular, the "cultural repertoire of meaning drawn upon by participants to define felt alterations in themselves" (p. ix). More generally, he seeks to "isolate and define the way in which an historical event of the first magnitude contributed to the character and definition of modernity" (p. ix). He concludes that "the war experience is an ultimate confirmation of the power of men to ascribe meaning and pattern to a world, even when the world seemed to resist all patterning" (p. x).

The war's structure and apparent autonomy produced in the individual soldier a sense of discontinuity of self and identity. There emerged what Leed denotes as "liminal man," a marginal character caught in the no man's land of the book's title between front and home, severed from society by mobilization and unable to reintegrate after demobilization. Adjustment to war was complicated by the intellectual baggage from the prewar period, above all, romantic perceptions of war as escape from modernity and purifier of bourgeois society. This vision, apparently realized in the class-negating "community of August" 1914, proved ephemeral: idealistic middle-class volunteers were frequently despised by working-class draftees, and utopian expectations were shattered by the grotesque realities of war—above all, the dominion of technology over men and the dilemma of trench warfare that victory required attack but defense was superior.

In response Leed perceives a defensive personality which sought to survive rather than win and directed hatred more against the war, home, and superiors than the enemy. One method of survival was fantasies and myth. Leed concurs with Fussell's comment: "That such a myth-ridden world could take shape in the midst of a war representing a triumph of modern industrialization, materialism and mechanism is an anomaly worth considering" (p. 115). Leed sees this anomaly explained by psychic need, myths being an "attempt to close the gap between the surprising realities of life and initial expectations" (p. 116). He describes a variety of fragmentary but widely shared visions of release—particularly the dream of flying—and nightmares of interment in a war underground (Fussell calls it "the troglodyte world") and Jünger's myth of war as a beneficent machine. The other "exit from the labyrinth" was neurosis, "a flight from an intolerable, destructive reality through illness" (p. 164). It was complicated by what Leed calls "the politics of neurosis," official rejection because of the threat to discipline and thus prosecution of war. A further difficulty was the minimal success of treatment through discipline, moralizing, and psychoanalysis. Yet neuroses were pervasive—some claimed univer-

sal—among front soldiers and not only did not disappear after the war but increased.

Veterans found it generally awkward, frequently impossible, to reintegrate into civilian society because of what Leed calls the collapse of “the economy of sacrifice”: wartime recognition of the soldier’s sacrifice of self was replaced by commercial values and the profiteer became the new model. The veteran was consequently confronted with the hypothetical choice of renouncing his vision to rejoin society or retaining his vision and remaining an alien in his own country. In reality, however, he had no choice since he had so internalized the war in the form of private myths and neuroses.

Yet these myths and neuroses did not remain private but helped to shape the interwar period. The First World War was not only a “nodal point in the history of industrial civilization” (p. 193) because material realities and traditional mentalities collided but also “the first holocaust” (p. 213) repeated in the Second World War. Leed thus makes it clear that war affected and was affected by modern culture. Indeed, war may be the dominant myth of modernity.

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ANTONELLO VENTURI. *Rivoluzionari russi in Italia, 1917–1921*. (Biblioteca di Storia Contemporanea. I Fatti e le Idee, Saggi e Biografie, number 424.) Milan: Feltrinelli. 1979. Pp. 265. L. 8,000.

Socialists who yelled “Abasso la guerra!” outside the gates of Fiat-Centro in March of 1917 began to hear not an echo but the response “Fare come in Russia!” Of course, no one in Turin or anywhere else in Italy knew just what that meant, for the news from Petrograd was spotty and its import uncertain. In the first few months the task of explaining Russian developments to readers of the leftist press was—regrettably—left to émigrés, always notoriously unreliable when it comes to explaining contemporary developments in the homeland.

It is customary for historians to speak of the “tragedy of Italian socialism,” uncommon to find them examining the Russian role in it. Three recent works have helped reduce our ignorance. Stefano Caretti’s *La rivoluzione russa e il socialismo italiano* (1974) and Helmut König’s *Lenin und der italienische Sozialismus* (1967) are both more meticulously researched than is Antonello Venturi’s book, and some readers will no doubt prefer their more cautious judgments. But the three studies complement each other more than they overlap, and Venturi’s, the newest, can be judged strictly on its own merits.

Venturi divides his book into four excessively long chapters, and it is easy to question his priorities. Far too much space (sixty-five pages) goes to V. V. Sukhomlin, a Socialist Revolutionary and political naïf who merely happened to be hanging around the offices of *Avanti!* when the February Revolution erupted in Russia. Sukhomlin understood that event imperfectly, and, in any event, he soon left for Petrograd. Michael Vodovosov, his undistinguished successor at the Socialist newspaper, sought to interpret the October Revolution; his method consisted largely of quoting Lenin. A more substantial figure was Nicholas Liubarsky, who had to deal with issues that were of overwhelming importance for Italian Socialists: Comintern membership and the question of the *consigli di fabbrica*. He also had the task of trying to bring Giacinto Menotti Serrati and Antonio Gramsci into the Soviet fold.

These latter issues and personalities receive Venturi’s close attention in the last, and best, third of this uneven work. He gives a good account of the important strike of April 1920 in Turin, and he deals sympathetically and intelligently with the sad Serrati mission to Russia. American readers will also be interested in Venturi’s discussion of Marc Slonim’s adventures in Italy.

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R. T. THOMAS. *Britain and Vichy: The Dilemma of Anglo-French Relations, 1940–42*. (The Making of the 20th Century.) New York: St. Martin’s Press. 1979. Pp. vii, 230. \$18.95.

“It is difficult to discern a clear pattern in Britain’s policy towards Vichy, or even make, in regard to that policy, a single statement which does not require endless qualifications” (pp. 178–79). The tone is almost regretful, yet in describing the confused and undirected efforts of the British government, R. T. Thomas has produced a highly coherent and well-written study.

The collapse of France in 1940 with the subsequent rise to power of Marshal Pétain presented Britain, as the author shows, with a situation for which it had no previous experience. Vichy was hardly a sovereign state, but it still had a fleet and colonies that the British wanted to keep out of the hands of the Germans. They also hoped to make Pétain and his cronies less collaborationist and eventually to bring France back into the war on their side.

The times were hardly propitious for normalizing relations, however, and shortly after the 1940 armistice, Britain’s justifiable fears about the future use

of the French fleet led to the attack at Mers-el-Kebir, which Thomas recognizes as inevitable but calls a blunder that "poisoned all future dealings between London and Vichy" (p. 46).

The book, in addition to discussing London's Vichy connection, also deals with the two other branches of British foreign policy: assisting the Free French movement in hopes that General de Gaulle would be able to rally the French Empire and negotiating with General Weygand, Vichy delegate general in North Africa, to try to coax North Africa into the war on the Allied side. "British policy vacillated between these three alternatives without finding a solution in exclusive pursuit of any of them," Thomas observes (p. 55).

Britain and Vichy falls into two parts: Britain's Vichy policy and Britain's response to the Vichy policy of the United States. Thomas shows that although the Americans shared Britain's overall aims, they were more eager to strike a deal with Vichy and were much more antipathetic to de Gaulle, so much so in fact that London feared that Roosevelt intended to make the Vichy regime the basis of the postwar government of France. Furthermore, while the British, after the defeat of Hitler, wanted to restore France to her status as a great power, the Americans wanted to keep the country weak and subservient. That American interests were not served in this way was due less to British objection than to the evolution of forces over which neither country had any control, for example, the murder of Admiral Darlan and the political skill of de Gaulle himself.

Both the British and Americans pursued policies that they hoped would better help them fight the war against Germany, but, as Thomas's study shows, the best course of action during a time of war might have the worst possible consequences after that war is over.

Britain and Vichy is a well-written study by one who has complete mastery of his resource materials. In handling well a difficult subject Thomas has maintained the high standards of the admirable "Making of the 20th Century" series in which his volume appears.

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LILLY MARCOU: *L'Internationale après Staline*. Paris: Bernard Grasset. 1979. Pp. 316.

When the fall of Khrushchev shook their doctrinal certainty, the Communist parties, as Lilly Marcou sees them, were "confrontés à une réalité diverse, diffuse, glissante, imprévisible" (p. 181). Most people find reality as insecure as this all of the time,

but Communists are different, and the historian has turned poet to capture their pathos. Other illuminations of this kind guide the reader through a labyrinthine passage of a dozen years in the history of world communism. The subject is the progression of international conferences, the "International" after the three Internationals, that tried to maintain unity following the death of Stalin and the disappearance of the Cominform. It is important, but only the touch of the poet keeps it from being profoundly dull.

The conferences declined from a tour de force of unity in 1957 to an admission of disunity in 1969. The Soviet Union was struggling against the spirit of national autonomy, in particular that spirit as an instrument of China's competitive claim upon world Communist leadership. At the first conference, China was supporting Russia; at the second, in 1960, it was reducing "le rôle dirigeant du Parti communiste de l'Union soviétique à une rôle historique et honorifique, plus que politique" (pp. 111-12); at the third and last conference, China was the enemy. To the author this split helped provide the conditions for the appearance of Eurocommunism, with its gentler, more reasonably hopeful promise.

One might object to the rigor of Lilly Marcou's self-discipline in keeping to the main issues and their protagonists. Her characters, Khrushchev, Brezhnev, Mao, Togliatti, Hoxha, Thorez, and others, are as select and lordly as the dramatis personae of a Racine play, and she might have inquired further into their social objectives and relation to the silent masses.

In her scholarly restraint, the author is more objective than her intent, which was to proceed, on the recommendation of Claude Lévi-Strauss, from a "lecture de gauche . . . avec l'âme de la pensée dialectique" (p. 14). Supplementing exhaustive research among the documents with interviews of living, talking participants, she has discovered as many of the essential facts as possible and let them tell the story without tenderness for her own *gauchisme*.

Properly seeking more enlightenment from history, Lilly Marcou goes back a century to examine Karl Marx's management of the First International. She finds in it a model resolution of the conflict between centralism and autonomy (p. 23). This encourages her hopefulness about the effects of the weakening of Soviet international control and the concomitant rise of Eurocommunism. But her ability to distinguish factuality from myth-making, evident throughout this study, lets her see the "force mythique" in Marx's International (p. 14). That mythical element has hidden the centralizing principles and iron will of Marx the leader, who broke up the organization rather than lose control of it to

anarchists and other elements demanding autonomy. Plus ça change. . .

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JOHN LUKACS. *1945: Year Zero*. New York: Doubleday. 1978. Pp. 322. \$8.95.

For John Lukacs, 1914 was the beginning of the end and 1945 the end of the end of the old era. A European-centered world was finally destroyed as barbarous Oriental hordes moved in from the East and well-intentioned, but naive, Americans marched in, a little too slowly, from the West. Lukacs's chatty series of essays is always literate, often provocative, and sometimes wrong. He devotes separate chapters to the main personalities of 1945—Hitler, a vulgar, brutal, shrewd nationalist; Churchill, who was almost always correct except when it came to his understanding of American policy; Roosevelt, weakened but with his Wilsonian illusions still intact; Stalin, a scheming, suspicious nationalist; and Truman, "a national blessing" (p. 134) who quickly grew with his job. He offers also an analysis of American opinion in his "Year Zero" and concludes with a moving memoir of Hungary in 1945 as the Nazis withdrew and the Russians took over.

Aside from the chapter on public opinion, Lukacs presents little evidence that he has spent much time with primary sources or even recent monographs. Further, his interpretations are not new. He covered the same ground in his *A History of the Cold War* (1961). According to Lukacs, the Cold War did not begin because of calculated Soviet or Russian imperialism. Stalin moved into a vacuum, and when the Americans did not react early on, as Churchill hoped they would, the Russian leader naturally made the most of the opportunity. If only a line had been drawn in 1942, the Iron Curtain would have clanked down much further east.

Yet this book is worth reading for the variety of penetrating insights Lukacs presents and for his ability to capture in a few phrases the dominant characteristics of an individual or a nation. Perhaps he overemphasizes the ethnic factor, whether it is the Oriental nature of the Russians or the multinational character of the United States. Characteristically, he was disturbed to find that Americans considered Hungary an Eastern European and not a Central European state.

Lukacs can be nasty, as in his gratuitous slur to countryman György Lukács (p. 297n) or in his contention that Communists in Hungary were generally ugly. Moreover, some of his notions are bizarre as in his claim that little has changed since 1945

with Norman Mailer still our "principal literary figure" (p. 242 n). He is surely off base in his impressionistic survey of magazine opinion, which is not informed by the latest scholarship. Even if we have to put up with an occasional bit of Nabokovian word play, however, it is worth waiting for such lines as these that describe Roosevelt: "When an American seductionist goes to the Caucasus he'd better bring plenty of money" (p. 93). *1945*, as with much of Lukacs's work, makes for interesting, if exasperating, reading. He has much to say that merits attention from those who seek to explain the origins of the Cold War.

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ALAN KREIDER. *English Chantries: The Road to Dissolution*. (Harvard Historical Studies, number 97.) Cambridge: Harvard University Press. 1979. Pp. xi, 279. \$20.00.

It was long believed that the effect of the Henrician and Edwardian Reformation at the parish level would always be beyond our ken due to lack of source material, but recently a new generation of historians has been showing what can be done, largely by using records that have long been known and used in other contexts. In the days when the dissolution of the monasteries loomed large, with interest centering on the dispersal of their great estates, on crown finance and administration, and on local power politics, the chantries with their tiny endowments seemed small beer indeed. But for the ordinary early-Tudor parishioner chantry priests were more familiar figures than abbots and their disappearance more noticeable on the local scene. Following in the footsteps of Christopher Kitching, Alan Kreider has provided the first treatment of the subject on a large scale. Eschewing the actual suppression of these essentially "intercessory institutions," the pensioning of their priests, and the sale of their property, he concentrates instead on attempting to discover what part chantries played during their two centuries of active life and on the run up to their dissolution.

Making full use of the chantry certificates and a great many other sources, Kreider is able to answer more fully than will ever be possible for monasteries the question as to the function of chantries in local society and hence the extent to which they were missed. Although they were established essentially to pray for the dead, Kreider finds that their priests performed a variety of other functions, acting as schoolmasters, auxiliary parish clergy, and dispensers of poor relief. He then proceeds to devote two chapters to the national debate concerning the

existence of purgatory, on the outcome of which the future of the chantries depended, and the book ends with detailed studies of the acts of 1545 and 1547. Although this is familiar ground and there are no startling revelations, it has never been set down so clearly or interpreted so effectively.

This is above all a very honest book. The author makes clear that his detailed findings relate only to four counties. It is to be hoped that similar studies will follow for other counties and that Kreider himself will give us another volume on the suppression and its consequences. He may well discover that the commissioners for the suppression, on whose findings he has so heavily relied, were not entirely objective in their assessments of the indispensability of the chantry personnel. If they followed the traditions established by the augmentations men who dissolved the monasteries, they followed a convenient rule of thumb: to disturb vested interests as little as possible, especially those covered by written agreements. As Kreider recognizes, speed was essential to the success of the operation, even if this entailed some loss of profit to the crown in the short run.

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W. J. SHEILS. *The Puritans in the Diocese of Peterborough, 1558-1610*. (Publications of the Northamptonshire Record Society, number 30.) Northampton: The Society. 1979. Pp. xi, 166. £7.50.

W. J. Sheils has added to the production of local Puritan geographical studies, in this case the diocese of Peterborough (coterminous with the counties of Northampton and Rutland) for the period of 1558-1610. His approach is to follow the outlines of the national history of Puritanism, primarily as laid out by Patrick Collinson in *The Elizabethan Puritan Movement* (1967), and then to examine the local manifestations of Puritanism. The choice of the Peterborough diocese for a new examination of Puritanism at the grass-roots level proves to be a good one because of the fertile growth experienced by Puritanism in the area. Among the notable Puritans with Peterborough connections were John Penry, who married into a Northampton family, Robert Browne, the original Brownist, William Hackett, the idealistic revolutionary, Peter Wentworth, the parliamentarian, Sir Walter Mildmay, and the Cecil family, to name a few. Browne settled at Thorpe Achurch in the diocese after returning from Dutch exile and renouncing his separatism. Even thereafter, he revealed several Nonconformist tendencies. During the enforcement of Bancroft's canons in 1604, sixteen clergy were deprived, the highest number of any diocese. One of the North-

ampton preachers described Puritans as "the hotter sort of Protestants," and Peterborough was well supplied with them.

Peterborough Puritanism, as described by Sheils, parallels quite closely the national trends. In the early Elizabethan years, the Puritan clergy operated through area exercises, fast days, and powerful preaching. From 1587 to 1591, the Puritan-minded clergy organized themselves into three classes, thus attempting "presbyterianism within episcopacy." Following the collapse of the classis movement, Sheils sees in the 1590s and beyond a shift to family and parishional Puritanism with the more modest goals of "non-separating congregationalism" (p. 68). He gives a valuable analytical as well as descriptive study of the deprivations resulting from the 1604 canons. Sheils calculates that 20 to 28 percent of the Peterborough clergy were actively opposed to the canons, about 12 percent were called before local courts as Nonconformists or involved in other Puritan activity against the canons, and 6 percent (16 clergy out of about 278) were actually deprived. Although the book cuts off at 1610, Sheils projects a few clues about the durability of the Puritan tradition over the next fifty years. Puritanism was still deeply entrenched at the time of the Restoration, and after 1660 the clergy from sixty-five parishes were silenced and removed.

This is a solidly researched book that throws considerable light on the local workings of Puritanism. Much of the book is descriptive and analytical on chronological lines; the last several chapters, however, contain some statistical and map analyses on clergy and gentry. Here and there the reader wishes for more. At one point a bare mention is made of possible radical influences from Dutch refugees at Stamford, but no follow-up is given. The topic of the social and economic program of Puritanism is briefly treated; an extension of this area would be welcome.

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SYBIL M. JACK. *Trade and Industry in Tudor and Stuart England*. (Historical Problems, Studies and Documents, number 27.) Winchester, Mass.: George Allen and Unwin. 1977. Pp. 200. Cloth \$29.95, paper \$12.50.

More than two-thirds of this book is a consideration of John U. Nef's long-dead thesis that an industrial revolution occurred in sixteenth- and seventeenth-century England, alleged by the publishers to be still controversial. The remainder (one-half according to the publishers who seem to be as incapable of counting as of judging historical controversies) con-

sists of thirty-four documents intended to illustrate the discussion. Sybil M. Jack's principles of selecting and editing these sources seem eccentric. One "document," number 25, headed "The Tanner's Costs," is not a single document at all but extracts from two; and it is not a statement of costs in one tanning firm but consists of records of bark sales to a tanner in Nottingham and of sales of fells and hides to a fellmonger and a tanner in Litchfield. Presumably, Jack is trying to illustrate a point about industrial costs made in the discussion, but in doing so she forces her sources into a Procrustean bed.

The main part of the book is interesting, less for what it tells us about Tudor and Stuart industry than for the methodological issues it raises. Jack states that "economic historians have always been principally concerned with growth and development or their absence" (p. 15). She assumes that sixteenth- and seventeenth-century industry must be discussed in the context of economic growth, and there has been no greater growth monger than Nef. But it is untrue that economic historians have *always* studied growth and questionable whether historians of the preindustrial period ought to be primarily concerned with it at all. Preindustrial economies almost by definition lack economic growth. Several years ago Postan suggested that historians of periods before the eighteenth century should be more concerned with the functioning of economies than with changes occurring over time.

Jack is of course aware that Nef's arguments were based upon atypical industries and exaggerated examples. She tries, therefore, to broaden the approach by examining sectorial shifts on the ground that economic growth involves the movement of resources from agriculture and personal services into industry and trade. One sympathizes with her willingness to use economic tools to examine economic history, but the concept of sectors, like that of growth, has been developed to describe modern, industry-based, capital-intensive economies. Its insensitive application to the early modern English economy, with its low levels of specialization, produces a good deal of convoluted debate leading to the banal conclusion that "significant change may have been structural." This statement is elaborated by claiming that "what was occurring concerned the changes which are necessary before revolution can come rather than revolution itself" (p. 115). If this means that the developments of the sixteenth and seventeenth centuries preceded those of the eighteenth and nineteenth, it is true but trite; but if it means that the Nef-type industrial revolution was a prerequisite for industrial changes in later periods, the statement is both logically and evidentially unsound.

It is a pity that Jack decided to cast her examina-

tion of Tudor and Stuart industry in the form she has, for the subject is important. She manages to convey quite a lot of useful information in her pursuit of the chimera of growth; but her discussion is confused by her conceptual apparatus and also by stylistic obscurities and numerous nonsequiturs that litter her text.

L. A. CLARKSON

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A. B. WORDEN, editor. *Edmund Ludlow: A Voyage from the Watch Tower*. Part 5, 1660-1662. (Camden Fourth Series, number 21.) London: Royal Historical Society. 1978. Pp. x, 370. £8.00.

As a result of A. B. Worden's edition of part 5 of *A Voyage from the Watch Tower*, the 1698-99 edition of Ludlow's *Memoirs*, one of the central texts of the Whig canon, can never again be read as it has been in the past. This newly discovered and original version of Ludlow's memoirs, of which part 5 is only a portion and which unfortunately covers only the post-1660 period of Ludlow's life, reappeared in 1970 and is now housed in the Bodleian. Students of the Restoration and of English republicanism in its pre-1660 form will be greatly assisted by Worden's editorial labors. He has done an admirable job of comparing the new text with the old and of identifying the somewhat significant alterations wrought upon the original manuscript by the editor of the 1698-99 text. Worden concludes that this hitherto unknown, although suspected, editor was John Toland (1670-1722). Although the evidence is by no means conclusive, I concur in his judgment.

In his lengthy introduction Worden marshals evidence for that judgment; he also attempts to place the appearance of Ludlow's *Memoirs* and Toland's editorial liberties in the context of the political issues of the late 1690s. His analysis of radical Whiggery in that period occasions the only reservations that must be voiced about what is, in every other way, a superb edition with a most useful introduction. Worden relates and indeed endorses much of the slanderous musing that surrounded the career of Toland, an infamous freethinker, republican, and, to use his own term, religious "pantheist." To fall prey to this propaganda would be to assume, as did some contemporaries, that Toland's religiosity was fraudulent and to fail therefore to see the relationship among his religious beliefs, his political projects, and his literary endeavors.

In his interpretation of Toland's context and motivation it would appear that Worden has allowed himself to be misled by the clergy. Of course they thought that they knew a great deal about this "incendiary," as the secretary of state called him. After

all, letters addressed to Toland turned up in the library of the Archbishop of Canterbury, Thomas Tenison, with the inscription "Letter to Mr Toland seized in Ireland" written across them. (Worden fails to note that Toland's own access to other people's mail was part of a two-way traffic.) But the historian should tread carefully here; the observations of contemporary enemies should be weighed against a careful reading of Toland's published and unpublished writings, especially his religious writings, since his consistency and originality primarily lie there. Toland's religiosity is of vital importance to any interpretation of his reasons for altering Ludlow's original text. As Worden rightly notes, Toland's doctoring was most ruthlessly surgical on those passages that reveal Ludlow's intense and radical Protestantism and, in particular, his millenarianism. In explicating Toland's alterations it is not sufficient to postulate that they are "a powerful reminder of the changes in belief and feeling which affected English society" in the later seventeenth century (pp. 51-52). Those changes, because they were incorporated into the political vision of the radical Whigs, require explication. And, once understood, they also provide the key to some of Toland's most historically important activities on both sides of the channel as well as to his profoundly historical cast of mind.

Having chosen to see Toland as somewhat disturbed (it is no longer fashionable to equate heresy with just plain sinfulness), Worden fails to understand that for Toland, as for Collins, the creation of a new natural religion, devoid of traditional Christian metaphysics and its priestly overseers, lay at the heart of his version of what should be the new Whig order in Church and state. And up until at least 1701 Toland was a valued member of "the same college of politicians," as Lord Methuen called it, which met among other places at the Grecian. No evidence from the 1690s suggests a separate "Roman" Whig faction, however closely allied with Toland's "Calves-Head" group, as Worden would have it. Rather, the evidence, including the Trenchard-Simpson correspondence at the Spencer Library (University of Kansas), which Worden understandably appears not to have seen, suggests no discernible distinction between these "politicians" (as one letter there speaks of Toland and Tindal) in the "college" and the crowd at the Grecian Tavern. Likewise, because Worden underestimates Toland he imagines that he was trying to rally the country—that is, Roman Whigs—to Calves-Head republicanism but inadvertently succeeded in transforming the latter into its muted version that became in the course of the eighteenth century the credo of the "country." It seems much more probable that this astute politician was intending some-

thing rather closer to what Worden describes as having happened.

In assessing the variety of motivations available to Toland as editor, Worden fails to grasp that he was also quite simply a historian with strongly antiquarian tendencies. In a letter to "a fellow collegian" written toward the end of his life, Toland saw himself as the successor of Anthony à Wood—an odd comment, given their political antipathies but perfectly understandable once Toland's significant historical and antiquarian research, particularly into the history of religions, is clearly interpreted and not ignored for the sake of pursuing his "purely" political activities.

Finally, it is mistaken to imagine the country Whigs, wedded to their estates in deepest wherever, as possessing "broader" horizons and greater intellectual depth than freethinkers like Toland. He and Collins were profoundly important religious thinkers whose forays onto the Continent implanted their ideas among circles of French Protestant refugees living in the Netherlands, some of whom went on to become leaders within European freemasonry. Although Ludlow requested that his text be edited to suit a new age, doubtless he would have strenuously objected to Toland's effort to make it speak not of "Christ ruling" but of virtues and ethics derived from stoicism and intended for adoption in a visionary republic of pantheists. Toland's republicanism was no less firmly rooted in religion than Ludlow's.

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JAMES DALY. *Sir Robert Filmer and English Political Thought*. Toronto: University of Toronto Press. 1979. Pp. xiv, 212. \$22.50.

Those of you who learned basic seventeenth-century English political thought more than twenty years ago must now certainly retrain yourselves. It used to be simple Whig versus Tory, royalist versus parliamentarian, absolutism versus liberty paradigms, easy to trace from Gooch or Allen. It was Hobbes, bad; Locke, good; and the test was simple Victorian liberalism. Not any more. From Pocock and Greenleaf to Kenyon and Schochet, all we have learned is that Stuart political morality, if it existed, had its own orientations that cannot be slighted merely because we fail to grasp their urgency.

We once knew Sir Robert Filmer as a quixotic Civil War royalist, author of *Patriarcha* (posthumously published in 1680), the absolutist absolutely vanquished by Locke's first treatise. But his

rehabilitation as a forceful theorist, first begun by Peter Laslett in 1949, more recently enhanced by Gordon Schochet and J. P. Kenyon, has been completed in this superb study by James Daly of McMaster University. In a style lucid, at times puckish, Daly takes us through a fresh version of Filmer, not so much by way of textual analysis (Daly virtually admits that Filmer was blindly single purposed) but by placing him in a world of wide-ranging possibilities and complex intellectual incentives. In an adroit presentation of Filmer's ideological alternatives, Daly presents in his second chapter alone a virtual compendium of seventeenth-century political ideas.

Among the author's numerous Filmerian attributions, we should now include Filmer as having played a part in the demolition of the *meum-et-tuum* world of the Elizabethans, which placed the state in cozy seclusion above society's petty preoccupations with private liberties. The state's totalist right might preempt anything in the private sector. Political adiaphora, the "things indifferent" of Hooker and Locke, were to Filmer without meaning. It was not only that patriarchal authority had been conferred on the state—the most familiar of Filmer's positions—but that the state was possessed of an omnipotent will, always capable of being translated into pure power. Daly's chapter on Filmer's royalist connections, admittedly his weakest, stresses the royalists' avoidance of his works, even during the exclusionist controversy. What we have in the bulk of the study, however, is not only a thorough delineation of mid-Stuart political ideas, one to be read as prelude to J. P. Kenyon's *Revolution Principles* (1977), which covers the ground after 1688, but also a study of man's relation to the state and to his own institutional being.

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BERNARD CAPP. *English Almanacs, 1500–1800: Astrology and the Popular Press*. Ithaca, N.Y.: Cornell University Press. 1979. Pp. 452. \$35.00.

Astrology, as Keith Thomas demonstrated in his *Religion and the Decline of Magic* (1971) and Bernard Capp confirms in this informative book, was a pervasive and flourishing cultural reality in Tudor and Stuart England. "It formed," says Capp, "a part of the dominant pattern of beliefs, though one which slowly declined and which coexisted very uneasily with other hostile elements" (p. 283). The almanacs of the period, in which astrology enjoyed a prominent position, sold up to four hundred thousand copies a year (in the later seventeenth century) and were undoubtedly consulted by a wide range of

readers. Astrology was of interest to both educated and poorly lettered people; it was a major component of popular literature; and the almanacs themselves—the central sources for Capp's enquiry—touched upon an impressive diversity of human concerns, concerns both for the workings of the heavens and for affairs on earth.

Capp's most important contribution lies in his detailed description and analysis of this (often colorful) diversity of concerns. Almanacs included political and religious prognostications and interpretive commentaries on public affairs. Their authors sometimes engaged in social criticism, and they commonly polemicized against corrupt clergymen, Puritans, and Roman Catholics. Astrology, in fact, was an important ideological weapon in the dissensions and intrigues of the period: it was employed to disarm and abuse enemies and to legitimize interests and objectives to which particular groups were already committed. It was also implicated in some of the millenarian thinking of these years.

Almanacs offered certain educational benefits as well. They helped to popularize natural science in a very elementary way: astrologers saw themselves as natural scientists, not as practitioners of the occult, and there was undoubtedly a substantial overlap between astrology and the more recent scientific knowledge. Almanacs served as calendars, helping people to discern with greater subtlety and discrimination the passage of time, and they provided some basic information on human history. They also offered to their readers a considerable volume of miscellaneous information of practical value—information about roads, fairs, farming, weights and measures, and the like. For many readers they were an accessible source of useful knowledge, some of it astrological (and meant to be used for practical effect) and some of it eminently mundane and unexotic. Almanacs, as Capp shows, cannot be categorized easily, for they mixed old beliefs (in magic) with new beliefs (in mechanical philosophy), they entertained with crude sensationalism and instructed with sensible advice, and they included both messianic prophecies and straightforward huckstering and special pleading. Taken as a whole, almanacs impinged upon, and drew their sustenance from, a very wide range of the experiences of the period.

Capp uses his evidence well. He provides an admirable summary and explication of the almanacs' contents, he tries to understand their character and functions within the context of the broader society, and he outlines the changes they underwent in the eighteenth century (which were partly a consequence of the declining confidence in the efficacy of astrological prognostications). He also pays close

attention to the publishing history of almanacs and the careers of their makers. His book is clearly organized and rich in detail, and it succeeds in casting new light on many well-cultivated, and a few less familiar, fields of early modern English history.

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PIERS MACKESY. *The Coward of Minden: The Affair of Lord George Sackville*. New York: St. Martin's Press. 1979. Pp. 279. \$17.95.

For the want of a cavalry charge at Minden on August 1, 1759, Prince Ferdinand's allied army lost the chance to turn a French retreat into a rout. Blame fell on Lord George Sackville, the commander of the British and Hanoverian squadrons, who, despite tradition and the title of Piers Mackesy's book, was hardly a coward. A court martial, however, found him guilty of disobedience and declared him unfit to serve in any military capacity. He recovered from his disgrace in George III's reign; by 1775, having changed his name to Germain, he became the American secretary and, according to Mackesy, bore with courage and intelligence the head load of the war.

Mackesy writes to explain Sackville's action on August first. His narrative agrees substantially with most modern authorities. Sackville was part of a complicated interplay of his own ambitions and England's factional politics, domestic and foreign. Following a quarrel with Ferdinand, Sackville found cause to question in the heat of battle what he believed to be confusing and conflicting orders—to advance to the left through woods he believed impassable and against an enemy he could not see but understood was to the front. His hesitation was also justified, holds Mackesy, because the prince, plagued by doubts, did not tell his subordinates his battle plans. (That caricature, along with many other assertions, needs a critical reading, because a 1761 letter, quoted on page 51 to prove Ferdinand's shortcomings, actually describes those of his nephew, Prince Charles.) As for the Sackville-Ferdinand quarrel, Mackesy offers no new insight or evidence. Nor is he able to penetrate the mystery of why Sackville apparently let pique keep him from reconnoitering the ground or obeying the orders that would have brought him the glory sought by every British officer in Germany. Possibly Lord George was in a state of mind similar to that of Cornwallis, whose refusal in 1780–81 to understand Clinton's clear orders lead to defeat at Yorktown.

Mackesy does, however, present the novel thesis that Sackville's homosexual orientation impaired his relations with others. The evidence, apart from patronage of two suspected homosexuals in the

1770s, comes from rumors, satirical poems, and gossip. Because Sackville's own correspondence file, "Minden Papers," has been missing for over one hundred and fifty years, the truth on many items may never be known. If Mackesy's suggestion is correct, however, it does not fit well with his desire to refute Sir John Fortescue's view that Sackville was a "deplorable" man.

An evaluation of Mackesy's extensive use of primary and secondary materials is impossible for anyone not intimately familiar with the sources because the author excluded reference notes to save space. Thus, *The Coward of Minden*, which might have discouraged further study for years, may in fact stimulate a new interest in Lord George.

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RALPH DAVIS. *The Industrial Revolution and British Overseas Trade*. Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. 135. Cloth \$18.00, paper \$9.00.

The subject of overseas trade as an influential factor in the social and economic growth and development of Great Britain and, in particular, the Industrial Revolution has long been debated in historical literature. This is not an unexpected result given the abundance of extant source materials. Nevertheless, overseas commerce has not been emphasized compared to other causal factors. One of the reasons for this has been the suspicious attitude that scholars have brought to the critical sources upon which depends the proper assessment of the value that overseas trade had on the British economy. Questions involving the balance of trade and payments, for example, have been thought unanswerable because the customs data in the reports of the inspector-general of imports and exports were recorded in "official values" rather than "current real values."

Among all the other questions that the late Ralph Davis and his associates have chosen to address, the computation of current real values for the period 1784–1856 may be considered one of their most important contributions. Great Britain's imports from the region "Northern Europe," for example, amounted to slightly more than an annual average of 10 percent of its total world imports in "official values" pounds sterling for the periods 1784–86 and 1794–96. Davis's computed current real values are, however, approximately 16 and 18 percent, respectively. This means that "Northern Europe" was extraordinarily important to the economic growth and development of Great Britain not only in terms of the enormous volume of imported raw and manufactured materials but also in terms of value.

Davis is not as precise in identifying other crucial

areas of scholarly concern. This less-than-comprehensive treatment may very well be owing to the magnitude of the problems identified in the title of the book and the author's death before a more thorough examination of the problems could be concluded. The reader, therefore, is presented with brief summaries of the components of the import, export, and re-export trade of Great Britain that highlight only a handful of composite commodities. Geographical areas are grouped together for the most part by region and not by country. Much of the chronology is devoted to the first half of the nineteenth century. Although there are adequate citations, there is no bibliography and only an incomplete explanation of the methods and sources used to generate the computed current real values. Finally, there seems to be an ambivalence in concluding on the overall influence of overseas trade on the British Industrial Revolution.

Despite these obvious and understandable shortcomings, Davis's study is a much-valued contribution. It is to be hoped that Davis's associates will continue to contribute more of their findings in article or book form or, at least, make them readily available to scholars who are still trying to unravel the complexities of Britain's international commerce.

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JAMES J. SACK. *The Grenvillites, 1801-29: Party Politics and Factionalism in the Age of Pitt and Liverpool*. Urbana: University of Illinois Press. 1979. Pp. xx, 244. \$12.50.

English politics in the early nineteenth century has hardly received the attention lavished upon the subject for the decades preceding the French Revolution or following Catholic Emancipation. Fluctuating between Namierite connection and an inchoate two-party system, politics in these years may alternately and sometimes simultaneously be considered in terms of Whig and Tory, court and country, or the machinations of Foxites, Pittites, Portland Whigs, Addingtonians, Grenvillites, and Canningites, associating in uneasy and usually fleeting alliances. This first full-length study of the Grenvillites, an important if elusive connection, is based on a wealth of manuscript material and is a welcome addition to the scholarship of the period.

James J. Sack distinguishes two groups among the Grenvillites, the family network and the broader parliamentary party. The former was directed from the family's country house at Stowe by the ambitious first Marquis of Buckingham until his death in 1813, when he was succeeded by his more ambitious son, Earl Temple, later first Duke

of Buckingham and Chandos. The family's influence in Bucks and Cornwall virtually assured their control of six parliamentary constituencies during the first marquis's lifetime, a nucleus enlarged by a train of nephews, in-laws, and hangers-on. In an excellent chapter, the author examines Buckingham's effective, if dubious, use of patronage and verifies the Grenvillites' reputation for venality.

The Grenvillite parliamentary group included the family connection, substantially augmented by politicians, particularly some prominent peers, who associated with Buckingham's younger brother, Lord Grenville. Having served as Pitt's foreign secretary from 1791 until the ministry's resignation in 1801, Grenville opposed the Addington and Pitt governments in the succeeding five years and became the reluctant leader of an influential parliamentary group in opposition. Sack calculates that in 1808, when strongest, the Grenvillite party consisted of twenty-four peers and twenty-two commoners. The bulk of the study is devoted to the forging of the Grenville-Foxite alliance, its brief Talents' Ministry of 1806-07, and the surprising endurance of that alliance in opposition until the schism of 1817 and Grenville's withdrawal from politics, which marked the effective end of the Grenvillite party. In two subsequent chapters, the author discusses the erosion of the remaining Grenvillite faction under the erratic Duke of Buckingham.

Sack excels in tracing the intricate maneuvering of both family and party and their ambiguous relationships to one another. But he does not overcome the tendency of this type of study to diminish political principle. While he repeatedly claims that many were attracted to Grenville's leadership because of ideological considerations, the ideology of Lord Grenville, much less that of the Grenvillite party, remains obscure. The reader does not learn why Grenville supported Catholic Emancipation. The Talents' Ministry's "one outstanding credit" (p. 95), the abolition of the slave trade, is given one line. Sack's analysis of the 1817 rift would have been enriched had he given greater consideration to conflicting ideas of liberty that obtained among Whigs and Grenvillites. Vestiges of the doctoral dissertation are evident in the excessive citations. Nevertheless, the study is useful and workmanlike and deserves the attention of specialists in the period.

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MICHAEL GLOVER. *A Very Slippery Fellow: The Life of Sir Robert Wilson, 1777-1849*. New York: Oxford University Press. 1978. Pp. xiii, 224. \$14.50.

Although not a major figure in the age of Pitt, Castlereagh, and Canning, Sir Robert Wilson deserves

another good biography. Based on Wilson's voluminous papers in the British Library and published works, Michael Glover's study supplements Giovanni Costigan's solid life of Wilson (1932).

Wilson was an erratic paradox. He fought against Napoleon for two decades only to become a partisan of the Napoleonic legend; he defended (in turgid prose) the autocracy of Alexander I and championed reform of Parliament. An incurable romantic, Wilson was never at a loss in self-advertisement, especially in exaggerating his military service and establishing himself as a military expert. His presence at Eylau, Friedland, and especially in the Peninsular campaign enabled Wilson to assert his claim as an authority on military affairs, even though Wellington denounced him as a disgrace to the British officer corps.

Returning to England in 1809, Wilson courted the Tory government for preferment while intriguing with the Whig opposition to make himself "an alternative hero to Wellington" (p. 82). As chief military adviser to the opposition, Wilson's gloomy prophesies on the Peninsular War were used by the Whigs against the government. But, as usual, his prognostications were consistently wrong and "classics of misinterpretation" (p. 83).

In 1812 Wilson was again in Russia and soon a spokesman for dissident Russian generals who demanded that the tsar dismiss his commander-in-chief, Kutuzov. Again, Wilson's wrongheadedness was astounding. A day after he asserted that Austria would not join the fray against Napoleon, Austria declared war against France. Similarly, Wilson foretold certain disaster for the allies at Leipzig, only to be confounded by Napoleon's defeat.

Surviving a disgraceful fiasco during the allied occupation of Paris, Wilson secured election to Parliament as a Radical in 1818, only to achieve a reputation as a parliamentary bore. Even more alarming to his parliamentary colleagues was Wilson's attachment to the Bonapartist cause, but this was counterbalanced by his advocacy of the cause of Queen Caroline against his *bête noire*, George IV.

Wilson was dismissed from the army and contemplated going to South America to lead the rebels against Spain. Deflected from his purpose by his wife's illness, Wilson went to Spain to help the Liberals resist French intervention. He set an example of bravery in a hopeless situation and returned to England one step ahead of Ferdinand VII's police. Wilson was now something of a hero and, veering away from his Radical friends, launched a campaign to recover his military rank. It was only after the death of George IV in 1830 that Robert Peel persuaded Wellington to permit restoration of Wilson's rank in the army. Having ruined his political career by equivocating during the Reform Bill de-

bates, Wilson gradually retired from politics. Melting with age, he now sought only a comfortable berth, which Peel provided by appointing Wilson Governor and Commander-in-Chief of Gibraltar in 1841. His six years on the Rock were quiet and "a fitting end to a long and turbulent life" (p. 196).

Glover's biography of Wilson is lucid, interesting, and enhanced by a useful bibliography and index. Unfortunately, it is ill served by an inconveniently placed footnote apparatus.

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DAVID ROBERTS. *Paternalism in Early Victorian England*. New Brunswick, N.J.: Rutgers University Press. 1979. Pp. x, 337, \$22.50.

It comes as something of a shock to realize that David Roberts's major study of early Victorian administrative history, *Victorian Origins of the British Welfare State*, was published as long ago as 1960. Now we have its successor—the book about which his friends have enquired and to which they have looked forward for some time. Roberts has returned to one of the themes mentioned, but not explored, in his earlier book. As so often in the writing of history, one good book has grown out of another. The idea of paternalism is familiar enough to students of the period. What Roberts has done is to take this concept and examine it in its various ramifications, particularly during the 1840s.

Paternalism in Early Victorian England is divided into three parts. Part 1 looks at the theory of paternalism as formulated by intellectuals. Between 1827, when Kenelm Digby published his *Broad Stone of Honour*, and 1847, the date of Arthur Helps's *Friends in Council*, more than thirty books espousing paternalist social ideas were published. In addition, novels, pamphlets, and articles championed the same principles. Roberts examines the reasons for this revival of older ideas and shows how they were disseminated by the Tory reviews.

The second part, "Paternalism at the Grass Roots," contains a valuable case study of Sussex, using local newspapers and poor law papers. The roles of landlords, clergy, and "captains of industry" are detailed in subsequent chapters. In part 3 Roberts analyzes the impact of paternalist ideas on politics and legislation. He shows that "since paternalism was a cluster of attitudes and ideas that revolved around differing attachments to private property, the Church of England, and various institutions of government" (p. 211) it produced several varieties of paternalist M.P.'s. These ranged from romantics like the Young Englanders to Peelites, Church of Englandites, and country squires. Paternalism as a literary and political force reached its peak of influ-

ence in 1844, argues Roberts. Thereafter it came to seem increasingly irrelevant. In the bourgeois England of post-1848 it appealed neither to the confident middle class nor to the radical, urban working men.

It is, of course, perfectly logical to study paternalism in the first instance through the paternalists themselves. This Roberts does and does excellently. The question for the historian, however, is whether this is enough. At times one almost feels that the author has become a prisoner of his own paternalists (having lived with them no doubt, off and on, for so long) and has lost the power to see another side of things. Roberts recognizes that deference is the obverse side of paternalism; but we hear almost nothing about deference from those who had to defer (or did not). History from below is notoriously difficult to write; but surely something needs to be said about paternalism from the receiving end? Although there are brief references to poor law riots, incendiarism, and Captain Swing, the full significance of these breaches in the wall of deference is not grasped. Nor is there any appreciation of urban radicalism in this period. In the summer of 1839 the Chartist *Northern Star* was selling fifty thousand copies—the largest circulation ever for a radical newspaper—and radical ideas penetrated into strongholds of northern paternalism. Roberts is too good a historian not to see the gap between the ideal of paternalism and the reality. But somehow the significance of the obverse dimension seems to have escaped him.

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JAMES H. TREBLE. *Urban Poverty in Britain, 1830–1914*. New York: St. Martin's Press. 1979. Pp. 216. \$20.00.

James H. Treble has provided us with a useful study of poverty in nineteenth-century Britain, but he blazes few new trails. He has combed the main printed sources most diligently and has incorporated some fresh material, particularly from Scotland, but his method is largely descriptive. In chapters 1 and 2, the best in the book, he brings together impressive quantities of evidence on movements in wage rates; he also provides a most skillful account of the importance of casual labor and structural underemployment to the problem of poverty. He proceeds by weight of example, which can make for dull reading on occasion, and he makes little attempt to compute his evidence and enter the minefield of debate on the extent to which poverty had diminished before the great upsurge of interest in the "social question" in the 1880s. Aware as one is of Treble's great knowledge about poverty and unemployment across the trades and throughout

Britain (though the mining districts receive relatively light treatment), it is disappointing to find a conclusion no sharper than this: "most of the available indices, qualitative as well as quantitative, suggest that primary poverty was more deeply entrenched and of wider dimensions and that the amount of secondary poverty was on no less a scale than by the time Booth and Rowntree had turned their attention to what, for them, was the burning social issue of the day" (p. 188). This sentence might also serve to demonstrate Treble's less-than-economical writing style.

It is also a drawback for those seeking raw dialectical meat that the author devotes relatively little space to the anterior causes of poverty. He is obviously right to stress casualty and underemployment but we are left uncertain why casualty was so prevalent in so many industries. The British economy was still immature in many respects, and entrepreneurs, probably to a greater extent than they realized, needed a large pool of underemployed labor on which they could draw at need (and at low wages) to trade competitively. We need to know more about the kind of economy that produced this kind of demoralizing poverty. Or, to take another of many possible lines, does Treble think that social historians have allowed the pendulum to swing too far in apportioning blame for the extent of poverty? Many Victorians could not see beyond individual improvidence; but do writers like Stedman Jones, in his generally excellent *Outcast London* (1971), play down the extent to which the poor made the worst of the unenviable circumstances they inherited? It is not necessary to swallow the individualist ethic of the Charity Organization Society to concede the basic point that far too many heads of household spent far too much time and money in the beer shop and visited the results of their improvident habits on their wives and children. Nor is it plausible to argue that the vortex of deprivation drew menfolk inexorably thither, since thousands avoided the snare.

The later chapters of the book contain little that is new. Readers will learn little about the operation of the poor law that they have not already gleaned from Michael Rose or from Derek Fraser's excellent symposium, *The New Poor Law in the Nineteenth Century* (1976). Nor does the material on diet take us far beyond John Burnett's immensely valuable *Plenty & Want* (1966). One wonders how far historians of late nineteenth- and early twentieth-century patterns of consumption can proceed without resort to oral history to flesh out skeletal sources.

I would like to end with two minor, but not insignificant, gripes. The book has been inadequately proofread. Some comical misprints survive; some 1946s should be 1846s; and, surely, proofreaders should not permit a misspelling of the author's

name to stand (p. 200, n. 192). Also, the publishers have resorted to the dubious, but increasingly common, economy of omitting a bibliography and making footnotes (collected at the back, of course) serve as "Sources and References." This is not good enough, and chasing a full reference back down a long chain containing *opere cit.* is far too time consuming as well. Authors inevitably use more works and collections than they have space to cite directly; other scholars often wish to know which.

ERIC J. EVANS
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DAVID ROBERTSON. *Sir Charles Eastlake and the Victorian Art World*. Princeton: Princeton University Press. 1978. Pp. xvii, 468. \$50.00.

The two pre-eminent figures in the "Victorian Art World," as David Robertson reasonably suggests, were John Ruskin and Sir Charles Eastlake. Whereas Ruskin stood outside the artistic establishment of Victorian England, Eastlake was at its very heart. The first part of this book traces Eastlake's achievements as a member and then president of the Royal Academy, as keeper and then director of the National Gallery, and as secretary of the Fine Arts Commission responsible for the interior decoration of the new Houses of Parliament. The operations of these official bodies are thoroughly researched and briskly described.

The greatest value of this book (and nearly half its considerable bulk) lies in its appendixes, which qualify it as a work of reference that students of the period cannot fail to find useful. Appendix D comprises a detailed catalogue of acquisitions made by the National Gallery from its inception until Eastlake's death in 1865; Appendix E provides a breathless, seventy-page summary of the principal pictures shown at the Royal Academy from 1850 to 1865, with excerpts from contemporary reviews. The bibliography of Victorian source materials is the most comprehensive yet compiled.

Eastlake's own paintings are little regarded today, but in cataloguing his exhibited pictures Robertson is unnecessarily apologetic about the space thus allotted to a minor artist. Eastlake's Italianate portraits and religious tableaux of 1835-45 were extremely well received by critics and patrons alike; his skill in evoking a formula exactly suited to the taste of the day is not to be underrated. In general, the author refrains from drawing conclusions from the facts he has so admirably marshaled. Eastlake's scholarly interest in the *quattrocentisti*, for example, and his desire to obtain their works for the National Gallery are clearly demonstrated; equally clear is the gallery's repeated failure to obtain such works while Eastlake held the post of keeper. Was

he unwilling, or unable, to communicate his enthusiasm and expertise to the trustees? One would like to know more of Eastlake as a personality. His marriage to Elizabeth Rigby—a marriage that, according to Samuel Rogers, astonished all Europe—is another topic into which the present book should provoke research. It is tempting to assume that the articulate, astringent Lady Eastlake, outspoken critic of *Jane Eyre* and *Modern Painters*, exerted a powerful influence on her less forceful husband. Although there is little in this study (or in Marion Lochhead's biography of Lady Eastlake) to confirm or refute our suspicions, illumination is surely to be found in the copious correspondence cited in the bibliography.

This book, however, is not offered primarily as a biography; Robertson describes it more accurately as "a piece of cultural history with a biographical centre." In documenting the National Gallery's purchases and omissions, the activities of the leading artists, and the violent controversies in the press and in Parliament, he has effectively illustrated the changing tastes of the eventful early years of Victoria's reign. Above all, he has opened up the field for future studies. Eastlake gave to his own magnum opus the typically self-effacing title of "Materials for a History of Oil Painting"; the present volume, which follows in the same traditions of scholarship and modesty, might as justly be entitled "Materials for a History of Victorian Art."

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LYNN HOLLEN LEES. *Exiles of Erin: Irish Migrants in Victorian London*. Ithaca, N.Y.: Cornell University Press. 1979. Pp. 276. \$16.50.

Exiles of Erin is an excellent synthesis of social-scientific theory, historical experience, and quantitative techniques. Lynn Hollen Lees reviews the complete experience of the Irish migrants who settled in London during the 1850s and 1860s. She examines their origins, the characteristics of their migration, the occupations they took up, the areas in which they lived, and their subsequent demographic, cultural, and political behavior. Lees has the happy knack of being able to write with the charm and style of the traditional historian while unobtrusively lacing her text with quantitative insights, the value of which even the most antinumerate among us must recognize and appreciate.

By 1861 there were 107,000 Irish men, women, and children living in London. Add to them the English-born children of Irish parents and one has a community of some 178,000. The migrants came

not from the most poverty-stricken and densely populated parts of Ireland but from relatively prosperous areas, where traditional economic and social structures were already crumbling. Almost half came as family groups straight to London. Once there, they were to be found throughout the city, not locked in an urban ghetto. Yet, while living close to the English, they remained apart, tending to concentrate in certain streets or courts, moving frequently (half the inhabitants of one court in 1861 were said to have moved in a month) though rarely going far. "At all ages and all stages of the life cycle" they held occupations demanding the lowest skills.

Contrary to the literary evidence the London Irish quickly adjusted their demographic behavior to that of the host population. Their age at marriage was soon virtually identical to that of the London population generally; so was their family and household size. As to the composition of the latter, "more lodgers but fewer servants and kin distinguished Irish from English." London did not destroy the Catholicism of the Irish. Admittedly church attendance was low in the 1850s, but this was in part due to the shortage of priests and churches. And in rural Ireland at this time church attendance was similarly low. As the nineteenth century advanced, however, church attendance increased, though it never reached in London the near universal attendance record of Ireland in 1900. Politically, the London Irish were soon extensively involved in Irish nationalist politics. By the end of the nineteenth century, however, they could be "mobilized in causes other than the defence of Ireland or the pope," although, according to Lees, "their activity in English-led, English-based political movements remained limited in London before 1914."

This brief summary of some of Lees's findings does scant justice to the wealth of detail that enlivens her narrative. In *Exiles of Erin* she adds greatly to our understanding of the first major migration of modern times. In doing so, she modifies existing theory, thereby helping anthropologists and sociologists to understand more fully the mass migrations that are such a feature of our own times.

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K. R. M. SHORT: *The Dynamite War: Irish-American Bombers in Victorian Britain*. Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. 278. \$13.50.

This thoroughly researched, detailed, and sound account of the bombing campaign carried out by terrorist groups emerging from the fragmented Fenian movement demands the attention both of specialists

in Irish history and experts concerned with terrorists. It can also be read as entertainment and will be mined by writers of historical fiction.

K. R. M. Short of Westminster College, Oxford, drew on all relevant police records in London and Dublin and, by special permission, had access to Scotland Yard and Home Office papers not due for release until the 1980s. With the aid of these, he weaves together the various threads of a complicated story into a coherent and interesting narrative.

After the failure of the Irish rising in 1867 and the Canadian raid of 1870, terrorism was the only card left to play by those Fenians who rejected legal and constitutional protest. Although agrarian secret societies and the Molly Maguires employed terrorism for economic protest, it had never been approved by Irish republicans from the time of Wolfe Tone to that of James Stephens.

The physical force wing of the Irish nationalists turned to bombing after spending several years in efforts to construct a submarine, which provided a useful contribution to naval architecture but not an effective commerce raider. With the adoption of a bombing campaign, the Clan na Gael and the less responsible Skirmishers of O'Donovan Rossa presented a major problem for the British police from 1881 until 1887. At that time, dynamite was a recent invention, opening new possibilities for terrorism that were being exploited by the Narodniki in contemporary Russia and would be exploited later by anarchists in Europe and the Industrial Workers of the World in America.

Short finds that the Clan na Gael, though not always the Skirmishers, were sensitive to public opinion. This limited what they could do, but the author believes that the Clan na Gael left the field undefeated, because the police were unable to develop a foolproof defense against bombing. The police, too, worked within the limitations imposed by political supervision and budget, but they enjoyed a large measure of success in uncovering diverse plots and placing obstacles in the way of conspiracy.

The efforts of the author to place the "Dynamite War" in its setting are generally effective but are marred by some astonishing errors of fact. On page 13 he writes that the rebellion of 1848 was a symbol of resistance during the potato famine. This would have been difficult as the worst years of the famine were 1845-46. On page 22 we are told that "the Canadian forces conclusively defeated the Fenians at the battle of Ridgeway," when, in fact, the Canadian militia was driven from the field by a Fenian bayonet charge. Such blemishes suggest that the author is more at home in the history of terrorism than in Irish and American history.

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A. J. A. MORRIS. *C. P. Trevelyan, 1870–1958: Portrait of a Radical*. Reprint. New York: St. Martin's Press. 1979. Pp. 193. \$19.95.

Convinced that Sir Edward Grey had lied about the extent of Britain's obligations to France, C. P. Trevelyan resigned from the Liberal government at the outbreak of World War I. His denunciation of British intervention proved to be a decisive moment in the march that would transform the Liberal Imperialist of the 1890s into the dedicated socialist of the 1920s.

Making very effective use of the Trevelyan family papers, A. J. A. Morris has analyzed the career of a politician who never achieved the highest offices but whose life illuminates the response of a part of the British political elite to the dissolution of the aristocratic-bourgeois synthesis at the turn of the century, the trauma of World War I, and the Great Depression. Occasionally, an excessive reliance on the Trevelyan papers gives this important short biography a too narrow focus.

Grand-nephew of Thomas Babington Macaulay and son of a prominent Gladstonian politician, Whig and Liberal blood flowed richly through the veins of C. P. Trevelyan. His youngest brother, George, would be regarded by many as the last of the great Whig historians. Born to privilege, the Trevelyan family believed that the advantages conferred upon them could only be justified by service to the nation. While this view can support a cruel paternalism, it led C. P. Trevelyan in a wholly different direction. Privilege—his own included—must go; equal opportunity for all must rise in its place.

Elected to the Commons in 1899, Trevelyan became Under Secretary at the Board of Education in 1908. Belonging to the radical section of the Liberal Party, he enthusiastically supported the social legislation of the pre-1914 years. Among the few to resign over opposition to the war, Trevelyan joined with E. D. Morel in forming the Union of Democratic Control, the most effective opponent of Britain's traditional foreign policy objectives during the conflict. His growing association with radicals, socialists, and trade unionists finally convinced Trevelyan that the qualitative changes required to create a more just and equitable society would never be realized by the Liberal Party.

Retaining many of the fine values of traditional liberalism, Trevelyan joined the Labour Party in 1918. While his commitment to specific socialist policies was often vague, he was quite precise about the need to expand educational opportunity for every citizen. He was a natural choice for President of the Board of Education in the first Labour cabinet in 1924 and again in 1929. Anticipating that Labour would act more courageously when it was

the largest party, Trevelyan was bitterly disappointed over the tepid support for his educational proposals. He resigned early in 1931, lamenting the failure of MacDonald to prescribe socialist medicine for the economic depression rather than vainly attempting to revive stale nostrums.

Defeated in the 1931 general election, he declined a new constituency on the grounds that room must be made for youth. Unfortunately, Morris gives only six pages to the last twenty-seven years of Trevelyan's life. It would have been beneficial to read more of Trevelyan's reaction to Labour's massive victory in 1945. One wonders if the old "radical" judged that Clement Attlee's government had moved Britain irreversibly in the direction of a just society or whether, once again, he found timidity and hesitancy winning out over socialist commitment.

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GISELA C. LEBZELTER. *Political Anti-Semitism in England, 1918–1939*. New York: Holmes and Meier. 1978. Pp. ix, 222. \$26.50.

The limited impact of modern anti-Semitism on political life in England stands in sharp contrast to its influence on the Continent. At the end of the nineteenth century, when anti-Semitic groups in Austria and Germany were achieving some success at the polls and the Dreyfus Affair was agitating France, the Jewish question in England failed to excite widespread popular interest and remained on the periphery of parliamentary politics. Only in the interwar years did it invade the national political arena, and even then its impact was circumscribed. Political anti-Semitism in England, nevertheless, remains an absorbing subject. Understanding its failure there and its success elsewhere makes apparent the essential differences between the social and political development of England and the Continent. Gisela C. Lebzelter's broadly conceived study of the eruption of political anti-Semitism in the 1920s and 1930s not only describes the nature and impact of the major anti-Semitic groups of the time and assesses the responses of Anglo-Jewry, the government, and the radical left but also addresses this larger question of why anti-Semitism never deeply penetrated English political life.

Lebzelter demonstrates that the failure of political anti-Semitism in England can hardly be due to the absence of radical anti-Semitic ideas. Publicists like Henry Hamilton Beamish, Arnold Spencer Leese, and William Joyce depicted the Jewish menace in terms as crude as those employed in Ger-

many. Their Manichean, racially oriented view of an inevitable life-and-death struggle between Jews and Aryans led them to advocate extreme solutions to the Jewish question, including, in the case of Leese, mass extermination.

The weakness of radical anti-Semitism, according to Lebzelter, can be attributed to the character of English political culture and the course of Anglo-Jewish history. There was no historical tradition of political anti-Semitism for anti-Jewish agitators to exploit. The acculturation and integration of Anglo-Jewry in the eighteenth and nineteenth centuries had proceeded far more smoothly than on the Continent. Emancipation never emerged as an important political issue nor did public Jew-baiting ever become respectable or fashionable. Moreover, the Jews were not the only distinctive minority in England. Other religious and ethnic minorities served as lightning rods for the hostility of the majority, while British cultural and racial superiority could be defined in opposition to nonwhite peoples under British colonial rule.

Thus, when Mosley's Fascists attacked Jews verbally and physically, they challenged mainstream English political culture, which condemned the use of physical violence and celebrated the virtues of legality, restraint, and compromise. Most Englishmen, Lebzelter points out, did not reject anti-Jewish prejudice as such, and, indeed, were tolerant of social and cultural expressions of anti-Semitism. What they rejected was the public exploitation of racial hatred for political ends. The government acted to curb the Fascists only when it realized that Jew-baiting was a threat to traditional political values. Some of these observations have been made previously, particularly by Robert Benewick, but Lebzelter's analysis is the most comprehensive to date.

This work will also serve as a valuable corrective to the treatment of Fascist anti-Semitism in Robert Skidelsky's biography of Mosley. There Skidelsky maintains that English Jewry's bitter opposition to British Fascism and German Nazism was largely to blame for the centrality of anti-Semitism in the Fascist program and that Mosley's proposals for the mass deportation of Anglo-Jewry were merely off-hand remarks, largely irrelevant to his program. Lebzelter demolishes this apology, pointing out that for Jews to oppose Hitler, even as early as 1933, was not unreasonable and that Jewish participation in anti-Fascist ranks did not justify Mosley's exploitation of racial prejudice. Finally, she establishes that Mosley's own "final solution" (a phrase he used repeatedly)—forcible expulsion—was not out of character with the substance of his attacks on English Jewry and, thus, should not be dismissed as an unfortunate consequence of heated elec-

tioning. In this and on other issues, Lebzelter's appraisal is judicious and balanced.

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F. H. HINSLEY. *British Intelligence in the Second World War: Its Influence on Strategy and Operations*. Volume 1. Assisted by E. E. THOMAS *et al.* New York: Cambridge University Press. 1979. Pp. xiii, 601. \$24.95.

This is the first volume of the British official history of the influence of intelligence upon the conduct of the Second World War. It shows the general state of the various intelligence agencies at the outbreak of the conflict and traces their development through the Norwegian campaign, the Battle of France, the Battle of Britain, the initial fluctuations of the Desert War in the Middle East, the campaign in Greece, and, as its final episode, the German attack upon Russia in June 1941. In the sense that the authors have had much fuller access to sources—some of which are still too sensitive to be revealed—than any previous writer in the field, the book may be regarded as the first authoritative study of its kind. In the extent to which it is comprehensive, covering as it does all kinds of intelligence from photographic reconnaissance to cipher breaking, it is entitled to the same description.

The chief conclusion to be drawn is that intelligence in general, and the new inferences to be drawn about it in the light of all this new information, exercised only a minor, and certainly not a decisive, effect upon the first two years of the war. It is true that Enigma gave Wavell very good information about Rommel's next moves in May 1941, but this did not make much difference to what happened. On a smaller scale, it is true that intelligence revealed the threat to *Glorious* in the Norwegian battle, but it did not prevent the sinking of the aircraft carrier. This conclusion may surprise some students of the war, especially those who are carried away by the academic hysteria that greeted the publication of the spate of popular histories following Donald McLachlan's initial incursion into naval intelligence. There is, after all, no need to re-write all war history as some had averred; there is only the need to write much of it in better and clearer terms.

Unfortunately, this last injunction must apply to this volume. The style of writing often falls below an acceptable and sometimes an understandable level. Jargon proliferates and the standard of scholarship is disappointing. Vast numbers of footnotes fail to indicate the nature of the sources, which is one of the essential ingredients of a source reference,

and some are misplaced in the text. There are even footnotes on footnotes, which is a habit that should be stamped out now before it becomes fashionable. The index is pathetic and the list of abbreviations, of which unnecessary numbers are employed, often leaves the reader blinded with science. These scholarly blemishes greatly mar the beginning of a most important control upon the history of the Second World War.

NOBLE FRANKLAND
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VERNON BOGDANOR. *Devolution*. (OPUS.) New York: Oxford University Press. 1979. Pp. 246. Cloth \$14.95, paper \$5.95.

This book constitutes the most intelligent statement on devolution to appear in the wake of the legislative measures promulgated by the government during the last Parliament. Unlike other studies, it is historical, comprehensive, and interpretive. Vernon Bogdanor examines successively attempts to devolve British governmental power from Gladstone's home rule bill in 1886 through the creation of Northern Ireland (the only area to experience devolution) in 1921 to the rise of Scottish and Welsh national movements in more recent years. But the emphasis of his study is on the present and the likely effect a creation of assemblies on the Celtic fringe will have on British national institutions and way of life. An important theory emerging from his analysis is that much of the current interest in devolution is a reaction to technocracy, corporatism, and other centralizing forces in the modern state.

Despite the attractiveness of devolution as an alternative to impersonal government and as a means to satisfy nationalist demands, there is much in Bogdanor's study to suggest that its application would be problematical at best. The creation of peripheral assemblies would ultimately raise questions about Britain's unitary government and the supremacy of Parliament. A reason why devolution has remained a "damp squib" since the time of Lloyd George has been the apathy of England, which contains 85 percent of the population, yet it would inevitably have far-reaching consequences on that country. Interestingly, the recent Labour government disregarded this fact and acted contrary to its centralist credo by fostering a legislative program that would sustain the support of the devolutionary parties in Parliament. Experience in Northern Ireland has shown, discounting sectarian strife, the advantages of devolving such areas as agriculture and industry, yet these are the very subjects not included in the latest proposals. Furthermore, there is no assurance that the addition of a regional tier (pursuant to the creation of a top layer by Britain's

entry into the European Economic Community) will make government any less remote from the people; it might even have the opposite effect. But the most serious flaw in the late government's devolution package, according to the author, is that it violates one of the canons of good government by separating spending power from the power to tax. This deficiency might provide an opening for more pernicious centrifugal movements and lead to eventual separation. But it is Bogdanor's view, inspired by Burkean and Gladstonian notions of nationality, that devolution need not lead to disunion and could very well result in closer union between England and its fringe areas. Furthermore, the recent legislation, however misguided, illogical, and dangerous, is consistent with Britain's tradition of carrying out radical changes in the operation of government while maintaining the essential form and spirit of its ancient constitution.

There is much in this study that is hypothetical, but it is based upon much solid reasoning and research. Clearly it distinguishes the author as an authority on devolution and supersedes Reginald Coupland's reliable *Welsh and Scottish Nationalism* (1954). But it seems unlikely, given the outcome of the devolution referenda in March 1979 and especially the general election in May, that the subject will attract great interest in the immediate future.

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PAUL BEW. *Land and the National Question in Ireland, 1858-82*. Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. 307. \$15.50.

In this impressive, detailed analysis of the Land League, Paul Bew claims that Barbara Solow's *The Land Question and the Irish Economy, 1870-1903* (1971) exaggerates the improved condition of rural Ireland from the early 1850s to the late 1870s. Increased grazing and continued emigration meant more cows and fewer people. In Connacht, pre-Famine patterns persisted: small farms, a surplus population in relation to economic opportunity, and strong reliance on the potato. When explaining the attraction of the Land League, Bew argues that James S. Donnelly, Jr.'s *The Land and People of Nineteenth Century Cork* (1975) and Joseph Lee's *The Modernization of Irish Society, 1848-1918* (1973) place too much emphasis on rising expectations as a factor in late nineteenth-century agrarian radicalism, and he insists that old nationalist aspirations and agrarian grievances remained as motivating forces. Bew makes an important point concerning the effect of Irish-America on rural consciousness in Ireland. Contact with American values through exiles gave the Irish

masses a political sophistication far beyond their economic and social situation.

Bew affirms Thomas N. Brown's *Irish-American Nationalism* (1966) thesis that the New York-designed "New Departure" strategy of 1879 was the beginning of the Land League. The New Departure projected a war on landlordism as a technique for mobilizing and radicalizing the Irish peasantry as a prelude to revolution. Although Parnell declined an invitation to serve as a constitutional front for Irish-American Republicans, he first embraced and then took command of the Land League, believing that a settlement of the agrarian issue would convert the Anglo-Irish Protestant gentry to nationalism. Although journalists emphasized boycotting, the most effective Land League action was "rent by bayonet": tenants supported by Irish-American money holding out to the last minute before paying rent, always negotiating reductions.

In his major thesis, Bew rejects the historical consensus concerning Land League harmony. There was a basic conflict of interests between its original constituency—the small farmers of the west—and the large farmers of Leinster and Munster who joined in 1880. The latter forced acceptance of the "Three F's" Land Act of 1881, a settlement that offered less than Parnell's hope for peasant proprietorship or Michael Davitt's goal of land nationalization after the achievement of an Irish state. Although it diminished landlordism, the Land Act left the problems of Connacht unsolved, perpetuating the specter of emigration.

Although he does not always write clearly and is too impressed with the views of Marx and Engels on Ireland, Bew has produced a well-researched, intelligent, and important book about a crucial phase of Irish history. He tells a great deal about the ability of Parnell to enlist all segments of Irish nationalism despite the vagueness of his intentions and the complicated interrelationships between the land and the national questions.

LAWRENCE J. MCCAFFREY
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P. W. J. RILEY. *The Union of England and Scotland: A Study in Anglo-Scottish Politics of the Eighteenth Century*. Manchester: Manchester University Press; distributed by Rowman and Littlefield, Totowa, N.J. 1979. Pp. xvi, 351. \$25.00.

People have long debated whether the 1707 incorporating union of England and Scotland has served well the interests of either nation. They will doubtless continue to do so. In the wake of P. W. J. Riley's new study, however, it is no longer possible to see the 1707 act as the culmination of a "natural"

historical development and its architects as men of vision.

As Riley details at length, the union resulted from concerns narrowly parochial and utterly cynical, which displayed slight regard for the needs of England and none at all for those of Scotland. Overwhelmingly, Scottish politicians in the early eighteenth century saw but one decisive objective: securing office and patronage for themselves and their allies while denying both to their rivals. To that end they were prepared to promote in the Scottish Parliament virtually any policy adopted by the court in London. Such politicians went into opposition not to vindicate Scottish patriotism or "country" principles but to create sufficient nuisance to compel the government to take them on board. Virtually no one challenged this arrangement or its central assumption, the English domination of Scottish policy, and Riley speculates that greater concern for Scotland's welfare would have developed only if the realm had remained a separate kingdom in the seventeenth century. But, whatever the reason, real power lay in London, and few Scottish parliamentarians ever imagined themselves challenging that fact: they sought office and its spoils rather than responsibility.

Nevertheless, the cutthroat competition among rival factions—Riley calls it a "jungle" (p. 6)—frequently made Scotland's governance a very difficult matter. The English court and Parliament displayed no greater high-mindedness, but the death of Anne's sole surviving heir made the problem of managing Scotland a matter of acute (if temporary) importance. Although few politicians would have stated it so starkly, Scotland in the early eighteenth century faced the grim alternatives of management or military conquest. Effective management required union, itself the product of a highly complex and fortuitous alliance of English politicians and mutually hostile Scottish groupings. The politics of union were utterly sordid, but on reflection Riley finds the alternative vastly more horrifying.

Within its terms Riley's analysis is unimpeachable, and his work visibly demolishes the traditional view, associated particularly with G. M. Trevelyan and G. S. Pryde, which portrays the union as inevitable and "natural." At the same time Riley's work vindicates at least broadly William Ferguson's recent study of Anglo-Scottish relations and the union.

The Union of England and Scotland runs into difficulty, however, when it encounters the realm of ideas. Fletcher of Saltoun fits uncomfortably within this matrix, and Riley is constrained to stress his uniqueness without ever doing justice to his ideas and the culture from which they derived. But the problem runs deeper than this. However obsessed men may be with patronage, however venal their

preoccupations, however pervasive a spoils system may be, then as now any such structure of politics will inherently require an accompanying structure of ideology. Mayor Daley and Paul Powell, no less than Queensberry and Argyll, inhabited a decidedly ideological world, and that world must be approached through more than the cackling malice of personal rivalry. It is now time to get beyond the poverty of Namierism and the politics of place to the increasingly classicized ideas that ever more powerfully informed even the most venal aspirations of the early eighteenth-century world. Riley has presented us with an able and detailed study but one greatly limited by its severely traditional assumptions.

ARTHUR H. WILLIAMSON
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KENNETH J. LOGUE. *Popular Disturbances in Scotland, 1780-1815*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. 278. \$18.50.

This is a detailed study based mainly on thorough research on the voluminous Scottish judicial papers. It contains accounts of all the disturbances that the author has been able to trace in this period, namely forty-two food riots, about forty antimilitia riots (all in August and September 1797), four others against recruitment, four against Highland clearances, seven political disturbances, six industrial ones, three over church appointments, four over turnpike toll-bars, and four others. As the first to attempt such an account, this is a useful work, especially the chapter on food riots (mostly over oatmeal) that confirms conclusions by students of other food riots about their relation to prices and the distribution network, lack of violence and pillage, and type of people involved.

Very much of the book consists of accounts of disturbance after disturbance and is sometimes heavy going. The author's constant attempts to go beyond narrative are hampered by the nature of his sources. Because his information is often patchy, he is forced to speculate. Newspapers are of little use to him.

Apart from the two chapters on food and militia riots, most of the book consists of disjointed episodes with little in common and gives no sense of continuity in attitudes and actions. One might well question whether "disturbances" really form a subject at all. The book studies "those disturbances which resulted in at least one person being charged with 'mobbing and rioting' as defined by the official prosecutor, either the local Procurator-Fiscal or an official in the Crown Office in Edinburgh. This has been done in order to have some sort of external criterion of what was or was not a popular distur-

bance" (p. 17). This seems an arbitrary definition, dependent not on the action itself but on the response of authorities, and means that the disturbances have no common unity but are a number of isolated events. The fact that he deals with industrial disturbances but not other industrial disputes underlines the fact that certain kinds of disturbance have more in common with other kinds of action than with other disturbances.

The value of the book lies in its account of individual disturbances. The concluding chapter makes some general conclusions, including that, of the mere 450 participants whose occupations are known, 65 percent were "skilled manual workers." The author has read widely on popular disturbances in general, but his lack of information hampers positive comparisons, and he does rather uncritically use terms like "pre-industrial," "watershed," "forward-looking," and "progressive" and assumes that modernization means a decline in violence.

IORWERTH PROTHERO
University of Manchester

IAN DONNACHIE. *A History of the Brewing Industry in Scotland*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xi, 287. \$34.50.

Scotland's brewing industry has long been the poor relation of the much more famous whisky trade. Ian Donnachie sets out to rescue the brewing industry from its historical oblivion. To what extent does he succeed? In the first seven chapters, which cover the period ca. 1650 to 1850 and deal with the origins of the industry, its expansion during the eighteenth century, cyclical fluctuations, the connection with agrarian improvement, capital investment, markets, and management and labor, Donnachie does very well indeed. He successfully surmounts one of the major difficulties of research into the industry—the absence of business papers—by a very thorough search of legal records, especially Court of Session sequestration papers. These provide considerable detail about individual brewing enterprises and flesh out the bare bones of the more readily available aggregate statistics. Such detail is most valuable when it is used in the chapters on agriculture and brewing and on entrepreneurship and capital to tackle important questions about Scotland's economic history. Thus future textbook writers will have little excuse for ignoring the contribution of brewery demand to agriculture, though they may still wonder exactly how much of the demand was satisfied from East Anglia rather than East Lothian and whether relations between brewers and farmers were quite so harmonious as this study implies

given the preponderance of barley prices in brewers' costs. They may have rather greater reservations about an estimate of capital formation that aggregates insurance valuations for the years 1793–1815 without any recognition of the price changes during the period and that assumes (without any attempt at justification) that "the average brewer undervalued his fixed assets in much the same way as cotton mill masters appear to have done" (p. 77). An appendix devoted to the methodology of capital formation estimates in the brewing industry would have been more useful than the list of valuations that is included.

The other five chapters deal with the industry from 1850 to 1977, with the majority of the discussion devoted to technological change, marketing (with a good account of the distinctive Scottish licensing system), and the brewing boom of the 1890s. The final chapter surveys the industry since 1914. Tracing the application of scientific methods in industry is never an easy task, but Donnachie's account seems a good deal more optimistic than earlier work by Sigsworth and by comparison with contemporary activity in the whisky industry. The use of substitutes for barley is attributed to science but arguably had rather more to do with the removal of protection for agriculture. The absence of business papers makes these chapters less satisfactory, for as the industry has become dominated by major firms important areas of business behavior such as pricing policy and competitive relationships become increasingly relevant to the overall structural change that Donnachie describes. Thus collusive behavior is not mentioned despite the existence of trade associations among both producers and retailers and (in 1969) a reference to the Monopolies Commission. Nevertheless, much of the book makes a useful contribution to Scottish industrial history.

RON WEIR
University of York

IAN CARTER. *Farmlife in Northeast Scotland, 1840–1914: The Poor Man's Country*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xiv, 258. \$28.75.

Extensive research in secondary literature has gone into this account of rural society in northeast Scotland. Despite the bucolic title, however, the text rapidly turns into a polemic on the "breaking of a peasant society." Ian Carter examines the relationships of farmers and workers in the different circumstances of the "Golden Age" and the "Great Depression." I have space for discussion of only two major points, the first of which concerns whether there was a distinct "peasantry" that late and whether it really was "broken." Peasants are defined as producing subsistence needs and entering

the market only to be able to pay rent and taxes. "Capitalist" farmers are defined as producing for the market. Of course, they lived partly off the farm, so that, in reality, there were "capitalist" farmers who had large holdings and whose family requirements were only a small fraction of total output, while "peasants" were those whose family consumption accounted for a large percentage of their total output. In short, a cline is indicated, not a categorical distinction that will bear sharp argument about class oppositions. The "breaking" of the farming ladder seems on the evidence presented to have been rather its voluntary dissolution by emigration toward higher incomes in the towns and colonies.

The author states that the northeastern Scottish peasantry "had not died without a fight" (p. 162), but he seems to strain to explain away the fact that the fighting did not follow class lines. He falls back on the assertion that the peasants did not mobilize support for their political interests because they were not smart enough to realize what these were and so let the "capitalist" farmers (mis)represent them. One becomes sceptical when Carter counts hired farm servants as peasants, duped by "capitalist" farmers into acting as their "shock troops."

The book is peppered with antipathies toward economists, rural sociologists, and above all University of Edinburgh historians. These last are accused of virtual conspiracy to present the polarized farmer/laborer society of the East Lothians as applying to all Lowland Scotland. More widely, according to Carter, this model has had the effect of making empirical investigation of regional variation in agricultural history appear unnecessary and has been used to justify conservative agrarian policies as far afield as the Third World. One cannot subscribe to that. British agricultural historians study little else except regional variations. W. W. Rostow lately came nearer the mark in accusing them of hiding behind Stephen Potter's ploy, "it's different in the South." Fifteen or twenty years ago studies of Norfolk and Lincolnshire were overinfluential in English, perhaps British, agricultural history, and I was myself a critic of the resultant anapisms. The cause, however, was nothing more sinister than the tendency of historians to follow the fashion and a mildly insensitive extrapolation from the known to the unknown. Subsequent revisionism, a sign of agricultural history's maturing, has converted the real problem into the opposite one of how to synthesize.

E. L. JONES
*La Trobe University and
University of Exeter*

FRANÇOIS CARON. *An Economic History of Modern France*. Translated by BARBARA BRAY. (Columbia Economic

History of the Modern World.) New York: Columbia University Press. 1979. Pp. 384. \$16.50.

François Caron divides his economic history of France into two parts: the first deals with the nineteenth century (1815–1914) and the second, and slightly longer, part with the years since 1914. This is the first work in English to give detailed treatment to the course of French economic history from 1914 until the mid-1970s. Other distinguishing features of this work are Caron's accent on growth and his de-emphasis of noneconomic factors, such as social attitudes. His generally positive judgments on the performance of the French economy during the nineteenth century and also during the 1920s are a reaction against the preoccupation of the last generation of Anglo-American scholars with the *mentalité* of the French businessman and with economic retardation. He discounts the view that the rapid economic growth of the 1950s and the 1960s was a sudden rupture with past stagnation, constituting a French "economic miracle." Instead, Caron views this growth as a logical prolongation of the industrial growth of the period 1896–1929, and he therefore calls into question the whole gamut of social and economic explanations that have been employed to explain French stagnation.

This work is less a narrative of events than an analysis of how and why the French economy performed as it did. Caron integrates the results of recent scholarship, including that of the French school of *histoire quantitative*, and succeeds admirably in presenting a balanced view of French economic growth. Avoiding a strict chronological approach, he analyzes separately such topics as population and the working force, productivity, banks, capital formation, foreign trade, agriculture, and industry. Some traditional subjects (for example, public finance) are neglected; there is little on tariff history but more on the balance of payments.

One central theme throughout is the role of the state. Even in the nineteenth century "the neutrality of the state was never anything but a theory" (p. 44) taught by liberal academics. The state's aims were contradictory in that it attempted to promote growth but avoid the social dislocations associated with it, to promote competition but not upset the status quo, and recently to encourage concentration, so French enterprises could compete with foreign multinationals, while favoring small- and medium-sized businesses over giant enterprise. Caron sees the present equilibrium between the public and private sectors as fragile, "and a slight modification strengthening the former would definitely turn France into a bureaucratic system" (p. 365).

In this new—there has been no French edition—and important work, Caron has been ill served by his translator and editor. The translator frequently

stumbles over the highly technical vocabulary. Her renderings of the French are often awkward and unclear and sometimes completely misleading. These, and the numerous errors in spelling and fact, should have been corrected during editing. Despite these defects, this is a welcome publication that deserves to reach a wider audience than its hard-cover format will permit. There are a large number of works on the economic history of modern Britain available in inexpensive editions but not one on France. This is unfortunate because, for those interested in the problems of development and models of growth, as much, if not more, may be learned from the French experience as from that of England.

CHARLES E. FREEDMAN
State University of New York,
Binghamton

PIERRE CAYEZ. *Métiers jacquard et hauts fourneaux aux origines de l'industrie lyonnaise*. Lyon: Presses Universitaires de Lyon. 1978. Pp. 464.

"Jacquard looms and blast furnaces"—it is hard to think of a less revealing title for this history of Lyon industry in the first century of the Industrial Revolution (actually 1780–1860). To be sure, silk weaving and metallurgy figure prominently, representing the traditional and the newer branches of industry respectively. Yet the emphasis has not been on technology, either for the bourgeois of Lyon then or for their current chronicler. Production in Lyon was dominated by commerce, later also by finance, with the silk merchant-manufacturer providing the model. In turn, the present study defines its scope in terms of an economic region, a space over which Lyon capital held sway. Pierre Cayez is far more concerned with decision making and market dominance, with the organizational and financial history of industry, than he is with technological or even purely economic aspects.

In other ways as well this is a somewhat curious book. The reason for its publication in present form is unclear. The volume represents the first half of a characteristically massive *thèse*, shorn of its scholarly apparatus but seemingly not otherwise modified. Some sentences sound almost absurd in the absence of footnotes, so clearly were they written as simple introductions to infrapaginal reviews of literature. A prefatory note informs us that the entire thesis is being reproduced by the University of Lille III; research libraries would gain from holding out for the integral version. Yet this volume will not appeal greatly to a wider audience either. The problem is not so much with the mass of factual material presented as with the insufficient intellectual inducement the reader is offered to work through that mass.

For the most part, this is very traditional industry history, more descriptive than analytical. It is of interest for two reasons. One is the large amount of primary material Cayez presents and has, presumably, unearthed (in the absence of footnotes one cannot be sure). He is cautious and critical regarding his sources and data, but 143 tables and 100 appended pages of charts, graphs, and maps cannot help but inform. The second merit of Cayez's work is that he opens the door on a number of important problems regarding industrialization in an urban region. Among them are the interaction of the urban economy with its regional and wider environment, particularly the extent to which the urban center imposes or fails to impose its own dynamic. Even more timely is the attention focused by the case of Lyon on the somewhat neglected urban aspects of protoindustrialization. Cities served as more than centers of shipping and commerce, foci of chiefly rural development. They and their immediate suburbs housed much production, often directly complementary to that of the surrounding rural areas. Finally, there is the crucial question of the transition from protoindustrial production to modern factory industry. The relations between the traditional and the newer industries, particularly at the level of capital and entrepreneurship, are Cayez's principal concerns.

The problem with the book is that exposition overwhelms analysis. The big questions are raised but only by way of introduction and summary. The data are not marshaled in support of contending hypotheses; rather, they parade by in tidy ranks. The reader is left to figure out for himself to what extent Cayez's research has shaken or reaffirmed accepted interpretations, cleared up or further complicated ongoing controversies. All too often, poor data (and some imprecision in presentation) leave no great sense of loss or disappointment: when no questions are asked, how can one fault the answers?

Yet, if any single city can serve as the microcosm of the French economy in the nineteenth century, surely Lyon is that city. For all its faults, Cayez's work does show us the often painful early steps and missteps in Lyon's evolution as a hinge between north and south in the economic geography of France.

PAUL M. HOHENBERG
Rensselaer Polytechnic Institute

CARL J. EKBERG. *The Failure of Louis XIV's Dutch War*. Chapel Hill: University of North Carolina Press. 1979. Pp. xix, 240. \$20.00.

Although France emerged from the Dutch War as the most feared state in Europe, the war had not gone according to French plans. What had begun

as a quick strike against the Dutch lasted for six years and led to the first European coalition to oppose Louis XIV. To understand more fully what went wrong, Carl J. Ekberg has made a detailed study of the months between the French failure to achieve total military victory over the Dutch in the campaign of 1672 and the withdrawal of French armies from Dutch territory in October 1673. It is a story of military and diplomatic failure and of how a limited war was transformed into a general European conflict. There are chapters dealing with various aspects of those months: the motives and person of the king, the conduct of the war, the failure of the Cologne conference, the widening of the war, and the growing isolation of France. By using memoirs of the period as well as the French war and foreign affairs archives to good effect, Ekberg has described the government's confusion and the king's inability to articulate a policy. Equally convincing is his picture of the king's excessive reliance on Louvois, frequently to the exclusion of his other ministers, and of Louvois's inadequacy once the neatly planned strategy of 1672 fell apart.

Ekberg is unrelenting in his indictment of Louis XIV, whom he sees, at least in these years, as governed by a consuming, highly personal, and puerile thirst for glory that led him to pursue a course of action contrary to the interests of the French state. Urged on by Louvois, Louis's egotism led him to ignore his other advisors who, according to Ekberg, were opposed to the war and its conduct and who advocated moderation in the tradition of Cardinal Mazarin. Stimulating as his assessment is, however, it has some serious problems. As his own evidence indicates, the documents do not support a case for opposition to royal policies within the government, except for Pomponne. Finally, we should not overlook the powerful element of continuity between Louis's reign and the past. However unwise, immoderate, even monomaniacal we find Louis XIV's behavior, his goals—given the greatly enhanced power of France—remained those of Richelieu and Mazarin.

RICHARD PLACE
Wayne State University

ROBERT SOUCY. *Fascist Intellectual: Drieu La Rochelle*. Berkeley and Los Angeles: University of California Press. 1979. Pp. x, 451. \$25.00.

Pierre Drieu La Rochelle was born in 1893 and died by suicide in 1945, having contemplated and attempted it on several earlier occasions. Between 1917 and his death he wrote some thirty books and pamphlets and two (unsuccessful) plays. Most of his novels were heavily autobiographical; two of them,

Reveuse bourgeoisie (1934) and *Gilles* (1939) are illuminating documents—the one on the bourgeois family (his own), the other on the development of a fascist intellectual (himself). I have never enjoyed his writing nor understood the widespread interest he generated in his lifetime and since. Perhaps, as with Faust's vision on the Brocken, many found in him an image of their own failed aspirations. Historians may see him as a convenient kaleidoscope, reflecting a *cursus* common to his generation: cult of Barrès and of self, surrealism, revolt against decadence, fascism (or communism, or a bit of both), suicide (or execution, or exile), and eventual institutionalization, in a variety of combinations. Drieu represents a failure carried to extremes that few of his companions—not Aragon, not Malraux—achieved yet typical enough to warrant generalization.

What could be more typical than this willowy, flanneled Anglomane, this flabby admirer of toughness, who inflated his schoolboy rebellion into intellectual arguments, projected his failings on others, railed at the decadence he brilliantly represented, glorified sports at which he was no good, rejected the materialism his hedonism required, turned his yearning for virility into a cult of what he lacked and his resentments into pranks, pamphlets, and spiteful creeds?

The procrastinating narcissist wanted to be a man of action. Political action was the noblest sport—more satisfying than his avid brothel-creeping. So Drieu magnified his egoism into elitist nationalism, flirted with royalism, coquetted with revolutionism, fell for fascism and clung to it because of (false) *noblesse oblige*, betrayed all his loves beginning with himself. Yet this spoiled child was clear-sighted enough to see his failures—of talent, affection, and commitment. He judged himself a weakling and a coward, knew himself to be as *taré* as the worst bourgeois. He was, Soucy quotes him, preoccupied with the fundamental problems of “action, sex and death” (pp. 231, 351). He found his action in sex, he took death for action. Every other page he wrote reflects the desperate realization that civilization was equivalent to impotence. For him. His hard, gem-like flame burnt down into mediocrity. Unlike many contemporaries, he knew it. At least, he said it. His published and unpublished writings are a continual denunciation of himself. We need not wonder at his destructiveness and his self-destruction.

Robert Soucy's book treats this complex figure with hard-headed sympathy. He gives us the rake's progress of an intellectual philanderer, whose fascism may indeed have been merely “the exasperation of a lazy mind” (p. 273). There may be other intellectual (and psychological) roads to fascism. But, if fascist intellectuals are those who cannot live

up to the world and to themselves, Soucy provides an awful warning for nonelite elitists.

EUGEN WEBER
University of California,
Los Angeles

JOSÉ MARAVALL. *Dictatorship and Political Dissent: Workers and Students in Franco's Spain*. New York: St. Martin's Press. 1978. Pp. 199. \$22.00.

Sociology in general, and especially political sociology, is a subject likely to encounter a multitude of obstacles within the repressive framework of a dictatorship. In fact, as far as Spain is concerned, it would not be preposterous to affirm that the knowledge yielded by the analysis of workers' and students' protests could be paralleled—admittedly in a more limited and elitist sphere—by a study of the research and writings of the generation of post-Civil War social scientists. Among them, José Maravall stands out in the specific fields of working-class problems and labor relations. His long dedication to these questions guarantees the value of his new contribution.

The book has as its central issue the why and how of political dissent under a capitalist dictatorship. Its case study is Franco's Spain, the dissenting voices those of the workers and the university students. The answer is partly to be found in the internal contradictions of a dictatorial system that, having started in the forties with a blend of political autocracy and economic autarchy, was later forced to accept more liberal and open policies to achieve growth and development. But, at the same time, it tried to preserve its authoritarian institutions. The tensions thus generated set the stage for the dissent of workers and students.

Dissent was partly determined by the dictatorship, but it also had its own causes and forces. Franco's regime was a kind of historical parenthesis between the Second Republic and the newly born democracy. Maravall searches for the connections between the protest of workers and students under Franco and the ideologies and organizations of the Republican period. He finds lines of continuity at all levels: professions, geographical areas, communities, even families—at least as far as the working class is concerned, for here group coherence and continuity is much greater than in the short experience of university life. But dissent in both worlds is closely related to the activities of left-wing political parties—PCE (Communists) and PSOE (Socialists) in particular. This leads to the conclusion that the politicization of dissent under a dictatorship is almost inevitable. Sometimes it will be subversive and clandestine, at other times it will infiltrate official

structures. Both strategies were tried in Spain without conclusive evidence in favor of either.

The lack of freedom under a dictatorship makes the study of any form of opposition rather difficult. Sources are always limited. Maravall adds to those easily available—the press, Ministry of Labor reports—some less well known—documents of illegal trade and students unions—and some totally original—personal interviews with workers' and students' leaders. The conclusions he reaches must be taken as indicative rather than definitive. There is a great deal of subjectivity in the supporting evidence of some protagonists. Maravall poses a lot of challenging questions and answers some of them. The book as a whole throws much light on some obscure areas of Franco's Spain. The possibility of extending the conclusions to other situations may, however, be limited given the exceptional nature of Franco's regime.

JOSE AMODIA
University of Bradford

JAMES D. TRACY. *The Politics of Erasmus: A Pacifist Intellectual and His Political Milieu*. Toronto: University of Toronto Press. 1978. Pp. 216. \$15.00.

No more than Shakespeare, Erasmus was a forgotten man during the centuries after his death. Although he lacked the advantage enjoyed by Luther and Calvin of having church historians dedicated to perpetuating his memory, he has always commanded the attention of scholars. The stream of publications rose enormously during the quincentennial celebration of his birth and has flowed on unabated, with more yet to come, as the magnificent new editions of his works progress in the Netherlands (in Latin) and in Toronto (in English). One of the most learned of Erasmus scholars is James D. Tracy, whose first book, *Erasmus: The Growth of a Mind*, provided a full chronological account of Erasmus's intellectual development. In his present volume Tracy focuses upon the realistic dimension of Erasmus's political thought.

Nearly all previous studies of Erasmus's political thought have worked with his treatises such as the *Panegyricus*, *Querela pacis*, and the *Institutio principis christiani*, they have dealt with perennial issues in political philosophy and have emphasized his moralism in contrast to the realism of contemporaries such as Machiavelli or even More. Like Preserved Smith, whose *Key to the Colloquies* sought to unlock the secret of the accretions and alterations of the text by identifying Erasmus's life circumstances at the time, Tracy explores correspondence and other documents to assess Erasmus's interest in and knowledge of actual military and political develop-

ments in the Netherlands as well as of diplomacy in high places in which influential friends of his were involved. A few examples of the concreteness added to the picture of Erasmus's thought by this detailed approach will be suggestive. In the *Querela pacis* Erasmus described mercenaries as the "vile excrement of criminality holding life less dear than a small bit of profit." Erasmus had a personal encounter with the notorious Black Band that explains the rancor he expressed. Erasmus's strong prejudices against the Habsburgs led him to blame them unfairly for many ills and his interpretation of the Guelders war was wildly wrong. But in both of these cases of bad political judgment Erasmus was reflecting views that had wide currency among his countrymen. Erasmus's observations on fiscal practices and political and military events are sufficiently accurate, when checked against other contemporary sources, to suggest that his letters are probably trustworthy even when there is no corroborating evidence. He saw quite clearly that the power of the great nobles and territorial princes was growing and that, as he wrote to Spalatin, "the remnants of ancient democracy" were passing from view.

Tracy concludes that Erasmus was well informed in politics and that he saw the state not just as a welfare institution but as a power that should defend the people, by force if necessary. He further shows that Erasmus was not just a cosmopolitan intellectual ("Where you fare well, there is your fatherland") but was for a time allied with the "national" party supportive of a specific *patria* and that he was not a mere moralist blind to power and hence unable to grasp the mainspring of relations between states. One wonders how the author justifies calling Erasmus a pacifist, given his recognition of the necessity of power and arms, if pacifism means opposition to war or to the use of military force for any purpose. This volume takes its place with other excellent recent Erasmus studies such as those of Marjorie O'Rourke Boyle, Franz Bierlaire, and Georges Chantraine.

LEWIS W. SPITZ
Stanford University

HERBERT H. ROWEN. *John de Witt, Grand Pensionary of Holland, 1625-1672*. Princeton: Princeton University Press. 1978. Pp. xiii, 949. \$40.00.

John De Witt has not been the subject of a full-scale biography since the 1880s: James Geddes's *History of the Administration of John de Witt* appeared in 1880 and an English translation of Antonin Lefèvre-Pontalis's *John de Witt* in 1885. Perhaps both of these works were prompted by the assurance,

proclaimed in 1872 by the distinguished Dutch statesman and historian J. R. Thorbecke, that anyone who gave us "a life of De Witt worthy of the man" would earn himself "a place among historians of all time" (p. vii). Geddes, however, never published more than his first volume. He may have been discouraged by what Robert Fruin said in his review: "To write a history of De Witt's times which satisfies the requirements of historical art as well as those of historical science is a very heavy task, especially for a foreigner" (p. vii).

How well has Herbert H. Rowen acquitted himself of this "very heavy task?" Let us first consider "the requirements of historical science." As counselor pensionary of Holland (1653-72), De Witt directed the foreign policy of the United Provinces; so his biographer must be a master of their relations with the France of Louis XIV and the England of Cromwell and of Charles II. De Witt was more than a modern "secretary of state," for there was no *stadholder*, president, or other monarch for him to report to; he was responsible to two collective masters: the States of Holland and the States General of the United Provinces. But the man who handled their incoming and outgoing correspondence also recommended policy, not only foreign but also financial, military, and naval policy and much else. In theory responsible to the States, in practice he was their manager. Rowen knows how the Dutch Republic worked, and he shows how De Witt gained control of the various aspects of government in chapters entitled "The Craft of Politics," "The Master of Patronage," "The Manager of State Finances," and "Diplomacy: Craft and Art." Readers may feel that the exposition of the Dutch constitutional system is not as clear as they would like it to be, but Rowen would reply that the unclarity is not in the eye of the expositor but in the system itself, as anyone who has tried to describe it knows.

Rowen has gone through the letters and papers of De Witt, and of the States of Holland and of the States General; the narrative rests upon the sources, and, indeed, it sometimes seems that nothing has been left out. There are 890 pages of text in 41 chapters. The list of printed sources runs to nine pages, not counting pamphlets and newspapers, which account for eight more. Then there are fifteen pages listing articles and books bearing on the subject. The requirements of historical science have been fulfilled.

What about the "requirements of historical art?" Rowen has the talent to write a charming chapter on life in The Hague, a moving chapter on De Witt's relations with his wife and children, a gripping account of the events leading up to his lynching in 1672. Art, too, has gone into the narrative of De Witt's role in naval strategy against England

and in military strategy against the invasion of Louis XIV. For Rowen, this has been a labor of love, and those who read the 890 pages will have gained a truer and more complete picture of De Witt than has ever before been presented.

One may nevertheless complain that it is not easy to find things in this book; it is indexed for names but not for subjects (of what use is a half column of references to the States General, with no sub-headings?). There are no maps, even to illustrate the descriptions of naval and military battles. There are no reproductions of portrait or bust, though these are described in detail. Perhaps the press is to blame for some of these curious omissions. This reader's principal suggestion, though, is that serious consideration be given to bringing out a book of one-third the length. Having set forth the record here with a Netherlandish love of detail, Rowen should now feel free, like Rembrandt, to paint a portrait that highlights the main features of his important subject.

GORDON GRIFFITHS
University of Washington

ANDRÉ CORDEWIENER. *Organisations politiques et milieux de presse en régime censitaire: L'Expérience liégeoise de 1830 à 1848*. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège, number 220.) Paris: Société d'Édition "Les Belles Lettres." 1978. Pp. 504, 19. fr. 150.

This remarkable regional study of Liège is concerned with synthesizing materials about "creative politics" in the initial years of Belgian independence as well as the formative role of the local press. André Cordewiener has not only extended the writings of P. Harsin and R. Demoulin on the instrumental part played by this industrial region in national politics, he has also documented more fully the causes for the breakdown of *unionisme* (and political romanticism) as the *liégeois* liberals and Catholics moved from cooperation to conflict. In a major historiographical sense, he has supplemented the efforts of H. Haag, A. Simon, and C. Lebas on political groups on the national level with a local study that is rich and exhaustive in its detail and documentation. Along with E. Witte in his seminal work *Politieke machtsstrijd in en om de voornaamste Belgische steden, 1830-1840* (1973), Cordewiener depicts and analyzes the bitter and prolonged postrevolution origins of the church-state issue by concentrating on the development and use of the press by the two major political organizations.

The work is more extensive and helpful on the liberal political groupings and opinions, especially their diversity, their increasing anticlericalism, and

the central force of freemasonry both *in* and *outside* their political organizations and the press. The regrouping of political forces is portrayed, with emphasis on a tight analysis of the composition, degree of cohesion, and points of discord of the liberals. This "party" factionalism and reconstruction in the forties demonstrates the importance of the schism between the "doctrinaire" and "progressive" variants of liberalism and its resolution. It furthermore allows the author to explain how and why moderation and caution won over the democratic ambitions of the more social "radicals and Progressives" in the intramural battle of liberals and how and why the reunited neoliberals chose the path of warfare against the Catholics and the church. Skillfully and sharply, the author distinguishes between politics centering on elections and their issues (which involved only a minority of participants because of the highly restrictive suffrage) and politics reflected by the press (which in these economic boom times had not only wide currency and appeal but also extensive influence). Cordewiener insists that both the division and redefinition of liberal positions in the forties was largely carried out in the *Journal de Liège*, *Liberal liégeoise*, *L'Espoir*, and *L'Industrie*, a process that, significantly, was not duplicated in the defensive Catholic press. His landmark history of this domestic political rearrangement depends upon a close study of the internal realignment of the Orangists, the episcopates' recondemnation of the freemasons, and the 1842–45 debates over education and freedom of the press.

One might question whether Cordewiener has proved that Liège moved Belgium in the direction of materialist, pragmatic but nonradical brand of liberalism. His work, however, illustrates that the demise of mixed ministries and coalition governments resulted in large part from left-wing liberal notions about separating state from church. The ecclesiastical control of education, as perceived by the Walloon bourgeoisie of the eastern industrial regions, was one major area of needed reform in addition to the abolition of the stamp duty on papers and more public assistance for new industries. The author also points to the diverse liberal newspapers as the "motor force" in disseminating these new ideas and promulgating the issues that resulted in not just a local victory but also national power for the liberals in 1846–48. Finally, the author has illuminated the crucial relationship of the press to embryonic parties in a very specific socioeconomic context and at a particular time when attracting broader support and building a definitive political program became a necessity.

This impressive scholarly work is not only based on a depth and range of documentary evidence that includes brochures, thirty newspapers, and state, lo-

cal, and private papers but also contains several significant appendixes of party and press persons of note.

PIERRE-HENRI LAURENT
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T. K. DERRY. *A History of Scandinavia: Norway, Sweden, Denmark, Finland and Iceland*. Minneapolis: University of Minnesota Press, in association with the American-Scandinavian Foundation. 1979. Pp. x, 447. \$19.75.

Those who feel the area we know as Scandinavia should be treated in a regional as well as in a national context have long awaited a good overview of the history of the five Nordic countries. They will welcome T. K. Derry's well-balanced treatment of the history of Scandinavian society from the pre-Viking period down to the Swedish electorate's (at least temporary) rejection of Social Democratic government in 1976. Devoting generous attention to cultural, economic, and social aspects of Scandinavian developments, Derry has admirably avoided reciting the details of dynastic and military entanglements that could easily be the death of an attempt at comparative history spanning such a long period and so many nation-states. Commendable, too, given the wealth of literature in English on the Viking era, is the author's concentration on the period since the late Middle Ages and the appearance of the nation-state in the north.

It is no easy task to weave the fabric of a "history of Scandinavia" from the research and writing of Nordic historians. Historians in all five countries dwell to a surprising and anachronistic extent upon their respective national societies (although these efforts are often methodologically or theoretically sophisticated), and broad comparative efforts have been limited to an abortive study of abandoned farms in the late Middle Ages, a recent study of Scandinavian responses to the Great Depression and a current study of the relationship between central governments and local communities in the five countries during the mid-eighteenth century. For this reason, Derry's constant efforts to place Scandinavia—taken as a whole—into the broader framework of European society are most refreshing. This may be illustrated by his consideration of the collective economic output of the twenty-two million Scandinavians alive in 1973 with that of other Western societies, including mention of grain production nearly equaling that of Great Britain and of a merchant marine six million tons larger than that of the British and more than twice the size of the American. Yet Derry is prudently aware that he must treat the region as one composed of five sepa-

rate societies, although they are linked together in interesting ways by the Nordic Council and related institutions.

The book is burdened with few inaccuracies, although the Finns may be irritated by the fact that the Finnish parliament, the Eduskunta, is consistently referred to as the "Elskunta," and readers referring to the end maps will despair at finding Sigtuna miles from Lake Mälaren and Elsinore dismayingly distant from its strategic location on the sound within sight of the Swedish town of Helsingborg. Without question, however, T. K. Derry's history of Scandinavia provides the student, the generalist, and the layman with a reliable, up-to-date, and highly readable introduction to the history of a region all too often ignored in Anglo-American treatments of early modern and, especially, modern Europe.

MICHAEL F. METCALF
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KEKKE STADIN. *Småstäder, småborgare och stora samhällsförändringar: Borgarnas sociala struktur i Arboga, Enköping och Västervik under perioden efter 1680* [Small Towns, the Petite Bourgeoisie, and the Great Social Transformation: Bourgeois Social Structure in Arboga, Enköping, and Västervik during the Period after 1680]. Summary in English. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 105.) Uppsala: Uppsala University; distributed by Almqvist and Wiksell International, Stockholm. 1979. Pp. 180. 66.50 KR.

Most social and economic research concerning late seventeenth-century Sweden has dealt with prominent and very prosperous elites. Kekke Stadin, however, has added significantly to our knowledge of this period through his study of the businessmen in three medium-sized towns from 1680 to 1715. The author examines this group to see if they experienced the same social and economic transformations then beginning in larger Swedish and European commercial centers. He looks particularly for evidence of capital accumulation and investment and for changes in the petite bourgeoisie's occupational structure and political and social influence.

Stadin chose his three towns on the basis of differing economic functions. Arboga was a commercial intermediary between the Bergslagen mine owners and Stockholm exporters. Enköping's economy was based upon the sale of foodstuffs to Stockholm. Västervik, unlike the others, sold its wood products, tar, and iron abroad, as well as to Stockholm. From his study, Stadin shows that an economic elite, smaller in number and more prosper-

ous than the upper ranks of the bourgeoisie had been before, began to emerge in the three towns. These merchants also tended to be established in the most progressive sectors of the towns' economies and to possess a greater number of employees than had been common previously.

Success was associated to a significant extent with the ability to adapt to new economic circumstances. Arboga's commerce declined severely when Bergslagen mine owners established direct commercial ties with Stockholm. Only small foundry owners were able to prosper because of the demand for their services fostered particularly by the Great Northern War. Västervik wholesalers were adversely affected by the establishment of a tar monopoly in Stockholm but adjusted by investing their capital in ship construction and ownership. When Enköping's fishing industry died out, the town's commercial farmers acquired even greater importance than they had possessed before. Stadin also shows that this emerging elite tended to secure a tighter grip upon the elective offices of local government and to strive toward greater social exclusiveness through restrictive marriage patterns.

Stadin presents an informative examination of the social and economic transformation of the petite bourgeoisie in these Swedish towns and establishes a pattern that he argues was true for other towns as well. The author uses his limited primary sources well and is obviously acquainted with the pertinent secondary materials in his field. Stadin establishes a reasonable definition of the group he studies without becoming entrapped in an excessive discourse on dogma and terminology. The book possesses a sufficient number of clear and explanatory tables and diagrams. It is blessed with a nine-page English summary that presents the essence of his project very well with only a handful of minor misspellings.

LELAND B. SATHER
Weber State College

ÅKE LINDSTRÖM. *Bruksarbetarfackföreningar: Metalls avdelningar vid bruken i östra Västmanlands län före 1911* [Swedish Ironworkers' Trade Unions: The Branches of the Swedish Engineering Union at the Ironworks in Eastern Västmanland before 1911]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 106.) Uppsala: Uppsala University; distributed by Almqvist and Wiksell International, Stockholm. 1979. Pp. 146. 58 KR.

The Swedes have achieved significant team-work in historical training and research. The well-known emigration project and the study of the family at Uppsala and World War II research at Stockholm have attracted twenty or more students each and

have provided group financial support. The team approach animates a theoretical framework within which individual researchers may investigate their segments of the subject and enjoy cooperation with other scholars. Unlike the seminar, this group activity usually extends over several years. The present study is the fourteenth publication within the project "The Functions of Swedish Class Society: Popular Movements," others being concerned with topics such as rural workers in Uppland, sports as a folk movement, political activities of popular movements in Gävle in the 1880s, strike-breaking and the freedom of labor, and conflicts within the Communist Party of Sweden. A variety of monographic material is thus handled within a program that has some cohesiveness of concept and methodology.

Åke Lindström's dissertation shows how labor's attitude toward unionization developed during the critical period 1900-10 in the rural ironworks of Västmanland in central Sweden, with special attention to Skultuna. Centuries-old patriarchal ironworks (*bruk*) had acquired a close-knit social and economic structure, a vertical solidarity embracing owner-manager, artisans, helpers, and families. Toward the end of the nineteenth century, new machinery, increased production, hiring of more workers from outside, and spreading labor consciousness led to changing attitudes of both owners and workers. Change was slower in rural *bruk* than in cities, as confirmed in related studies in this series. Change came most slowly in the smaller *bruk*, where relationships of owner and worker were more intimate. In eight of the smallest *bruk* in Västmanland no unions were established by 1910. But many *bruk* were becoming consolidated, so that nationally the number declined from 381 to 140 (1870-1913). Frequently the patriarch-owner was displaced by a newcomer superintendent (*disponent*). Gradually a horizontal solidarity of workers replaced the older vertical solidarity of the *bruk*.

Differences are pointed out between classes of workers: for example, the smiths formed a self-conscious elite among the ironworkers, and because of pride and privilege they were most hesitant to join the union of metalworkers; they clung to older methods of work and reacted against innovative techniques as well as against innovative trade associations. Unionization came largely through quiet personal persuasion without mass meetings or confrontation. And superintendents reacted with restraint—a few firings, some thwarted attempts to charge rent for previously free housing, and little more. A new day had arrived and was accepted. In 1900 only 6 percent of Västmanland's ironworkers were organized; in 1910 the figure was 54 percent.

The eight-page English summary is excellent, the 112-page Swedish text is well organized, although

unnecessarily repetitious. Here in a microcosm of time and place is a valuable account of the Swedish shift from patriarchy to unionism.

FRANKLIN D. SCOTT
Honnold Library,
Claremont

ROBERT SCHWEITZER. *Autonomie und Autokratie: Die Stellung des Grossfürstentums Finnland im russischen Reich in der zweiten Hälfte des 19. Jahrhunderts (1863-1899)*. (Marburger Abhandlungen zur Geschichte und Kultur Osteuropas, number 19.) Giessen: Wilhelm Schmitz Verlag. 1978. Pp. vii, 395, DM 80.

If one of the most important functions of the historian is to call into question long-held, comfortable assumptions and to reveal the complexities of persons, events, and institutions, the author of the present volume deserves unstinted praise for his remarkable erudition and analytical skill. Using a wealth of archival materials in the Soviet Union and Finland and scholarly and not-so-scholarly studies in six languages, Robert Schweitzer has produced a work of first-rate importance for understanding the relationship between tsarist Russia and its non-Russian subjects.

The subject of attempted Russification in the western borderlands from Alexander II to the First World War has been the subject of plausible generalizations for many years. To test such generalizations the author uses as a case study Finland, which, as a Grand Duchy, had achieved by the late nineteenth century by far the highest level of self-government of any of the Romanov dominions. He has explored some of these generalizations with a thoroughness that even the ablest Finnish scholars have not equaled and has attacked some assumptions with considerable success.

It has been customary, for instance, to attribute the tougher policy of St. Petersburg toward Finland in the last two decades of the nineteenth century to the increasingly reactionary atmosphere of the imperial government and the fear that a liberal infection might spread from the Grand Duchy. The author disputes this interpretation: "Finland's autonomy was not directly dependent upon the degree of liberalism in Russian internal policy, and the Finnish constitution was not such a tempting example for the Russian opposition that Alexander III would have had to liquidate it for the safety of the autocracy. The change of rulers brought, it is true, a personal shift in policy toward Finland, but no new basic program" (p. 9). Indeed, as Schweitzer illustrates by numerous examples, Alexander III was not consistently, or even basically, a foe of the Finnish constitutional system. Insofar as it was con-

venient, he was willing to let sleeping dogs lie and to disregard autocratic dogma with respect to the Grand Duchy. Circumstances, however, forced a confrontation that the tsar would have preferred to avoid.

The nature and scope of Finland's "constitution" in the nineteenth century has always been wrapped in ambiguity. As one minister state secretary for Finland put it in the middle of the century, "The Finnish constitution is like the illicit relationship of a married man. Everyone knows about it; everyone accepts it; but the less it is talked about the more happily the partners can get along together." Schweitzer does not cite this dictum, but he probably would agree with it. One of the most striking facts to emerge from these pages is the incessant talking about the Russo-Finnish relationship that went on from the mid-nineteenth century onward, as well as the growing acerbity of the dialogue. As long as Finland's position remained hazy and Russian policy toward Finland was determined by quiet personal contacts in St. Petersburg, things went reasonably well. For various reasons, however—including pressure from Russian jurists and military men—the imperial government found it necessary in the last quarter of the century to reach a more exact formulation of the Grand Duchy's position. "From the political point of view," the author writes, "Finland's status had never had any other basis than the will of the tsar. Alexander III, however, was the first to regard himself as compelled to determine this juridically" (p. 288). Out of this apparent necessity arose conflicts of opinion that eventually became irreconcilable.

The author traces exhaustively—even exhaustingly—the complexities of the Finnish problem from the mid-nineteenth century to the eve of the February Manifesto of 1899, which inaugurated open resistance on the part of Finns. In this detailed analysis no one, it seems clear, could possibly have done a better job than Schweitzer has. It would seem churlish, then, to complain of omissions in this remarkable exegesis. One should note, however, that Schweitzer's study remains deliberately on the theoretical, juridical, and political level. It is a little like some old-fashioned diplomatic history in that it describes the claims, the negotiations, and the re-creations but does not consider some of the pressures on both sides that pushed the participants into their theoretical positions. One of these pressures was the concern of Russian military men about the vulnerability of St. Petersburg; unsure of Finnish loyalty, they were determined that Finland should not become the springboard for an attack on the capital. Another was the slow but sweeping transformation taking place in Finnish society, which, among other things, increased the self-assertiveness of the Finnish elites and, simultaneously, their fear

that an erosion of their constitutional status might lead to a loss of their social privileges.

Such considerations, however, are for another book. Within the limits he has set for himself, Schweitzer has produced a work that can truly be called definitive.

C. LEONARD LUNDIN
Indiana University,
Bloomington

HEIKKI RANTATUPA. *Elintarvikehuolto ja -säännöstely Suomessa vuosina, 1914-1921* [The Provisioning and Rationing of Food in Finland, 1914-21]. Summary in French. (*Studia Historica Jyväskyläensia*, number 17). Jyväskylä: Jyväskylän Yliopisto. 1979. Pp. 276.

The circumstances surrounding Finland's declaration of independence in 1917 and the brief intense civil war of 1918 are gradually acquiring historical documentation. To cite only a few examples, there are the studies of the international diplomatic context by Yrjö Nurmio and Tuomo Polvinen, the research by Jaakko Paavolainen on Red and White terrorist atrocities in 1918 and Vilho Rasila's attempt to correlate socioeconomic variables with political behavior at the village level. What scholars such as these have contributed, beyond their data, is a conceptual framework facilitating the examination of highly charged events in a relatively objective comparative framework.

Heikki Rantatupa's doctoral dissertation is also based on this approach. His research question is straightforward: How did Finland's governmental authorities attempt to provide food for Finnish civilians from 1914 to 1921, and how did their plans work out in practice? The core of his data comes from the surviving archives of successive central food administration agencies, including Oskari Tokoi's socialist commissariat during the spring of 1918. His methodology is descriptive, though incorporating statistics wherever the uneven records permit such treatment. His results account for variations both over time and in geographical regions, often illustrated by maps.

As of 1914, Finland imported 60 percent of its food grain supplies, mainly from Russia, to feed its 3.3 million people. Through 1916 Russian supplies were available and only price controls were applied in Finland. From early 1917 Russian imports dwindled, and from the summer of 1917 to the end of 1918 Finns lived almost entirely on domestic food stocks, with minimal supplements from Russia and later from Germany. Food rationing and compulsory crop sales to government agencies were legislated in the spring of 1917 and remained in force until the spring of 1920.

Under the consumption norms set in 1917, farm-

ing areas in south and west Finland were producing a surplus, and the task of central authorities was to transfer this surplus to two deficit groups, the 30 percent of the population living in urban areas and the residents of rural eastern and northern Finnish provinces not normally self-sufficient, especially in grains. The cities had first access to whatever imports could be secured, but in the countryside the central authorities were unable to bring about any substantial redistribution of food supplies. In fact, though not in law, food in rural areas was controlled by local governments with local needs in mind, whether it was protecting a surplus or seeking to make up a deficit. The most acute periods of malnutrition came in mid-1918, after the civil war and before the autumn harvest. Generally, the food policies of Red and White governments did not differ sharply, save that military needs were given higher official priority on the White side, and the Reds had larger urban populations to feed. Only access to American grain supplies after January 1919 finally lifted the threat of starvation.

Rantatupa does not directly assess the broader significance of food supply policies for other events during these years, yet some implications are obvious. No central agency in Finland possessed administrative tools strong enough to force local governments to comply with national policy. This local nature of actual food management, especially in the countryside, needs to be appreciated in estimating the role of food shortages in precipitating the civil war. At the level of international diplomacy the desperate need for food imports from Russia, Germany, and the Allies has already been shown to be important. Rantatupa's careful account only underlines this.

The French summary is useful not only for those unable to read Finnish but also for others who may lose the thread of argument from time to time in the details of the original text. Indexes of personal and place names are included.

ROBERTA G. SELLECK
Harvard University Library

ULRICH MARWEDEL. *Carl von Clausewitz: Persönlichkeit und Wirkungsgeschichte seines Werkes bis 1918*. (Militär-geschichtlichen Studien, number 25.) Boppard a. R.: Harald Boldt Verlag. 1978. Pp. viii, 296.

This book is primarily valuable for demonstrating conclusively several points so familiar to modern Clausewitz scholars that no one has bothered to discuss them in detail. It title promises more than the author delivers. Clausewitz's influence outside of Germany is discussed in a twenty-five page appendix—an arguable distortion of emphasis. The bulk of Ulrich Marwedel's book considers the evaluation

of Clausewitz's career and work by German contemporaries and successors.

Within these limits Marwedel provides a useful summary of Clausewitz's professional and intellectual activity. It compensates in brevity and readability for a certain lack of depth; *Carl von Clausewitz* does not compare to Peter Paret's *Clausewitz and the State* (1976) in its analysis of Clausewitz's thought. Marwedel's strength instead lies in his treatment of Clausewitz's reception. Particularly after temporarily entering Russian service, Clausewitz acquired an enduring reputation for unreliability. His contributions to Prussia's military reforms were overlooked. He never received assignments that might have established him as a major field commander. Instead, almost by default, he emerged as Prussia's and Germany's great theorist of war. Unfortunately, Clausewitz's intellectual heritage was more praised than comprehended. Marwedel correctly describes this as reflecting a general failure to understand and work through the dialectical method essential to Clausewitz's thought. The men of a realistic, practical age rejected philosophic underpinnings and instead sought concrete rules of behavior. For them Clausewitz became a source of formulas and precepts, a recipe book for victory—if only one could understand the recipes.

The result of this approach was the scrambling or distorting of almost every one of the master's essential points. Even Clausewitz's stressing of moral and psychological factors led to an emphasis on cultivating inherited traits of character, as opposed to developing the intellect and the personality. Absolute war and the battle of annihilation became shibboleths; arguments for limiting and controlling war were ignored or explained away. Even on a practical level Clausewitz's epigoni so eagerly absorbed his criticisms of defensive warfare that they ignored the possible impact of an improving technology on the conduct of operations. The Wars of Liberation became a military model, an endless source of first principles. The result was a mind-set equally hostile to history and the dialectic. Clausewitz, Marwedel argues convincingly, was not an inspiration for German theories of war. He became instead a source of quotations for preconceptions—a pattern shaken, but not destroyed, by the events of World War I.

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Colorado College

WARREN B. MORRIS, JR. *The Road To Olmütz: The Career of Joseph Maria von Radowitz*. New York: Revisionist Press. 1976. Pp. v, 185. \$44.95.

This political biography of Joseph Maria von Radowitz provides a brief overview of the career of one of the more remarkable German statesmen of the

nineteenth century. Although it is based primarily on published sources and adds little to what is already known about Radowitz, it helps fill a void in the literature by giving the English-speaking reader a scholarly introduction to the life and work of the man who was the principal author of the Prussian Union Project of 1849–50.

A devout Roman Catholic whose family had moved from Hungary to Saxony in the eighteenth century, Radowitz had a career that began with a position in the military academy of Electoral Hesse and ended with a brief but significant tenure as foreign minister of Prussia. He first distinguished himself as an artillery expert and became chief of the Prussian general staff of the artillery at the age of thirty-three. Subsequently, he was the Prussian representative to the military commission of the German Confederation, Prussian minister to Baden, and leader of the conservative faction in the Frankfurt Parliament in 1848. A moderate conservative, Radowitz's political goal was to substitute a tight federation of the German states under Prussian domination for the Austrian-led Confederation of 1815.

The author provides some sound judgments on Radowitz's career. His rapid rise in the Prussian service is properly attributed to luck, unusually good connections (especially his friendship with Frederick William IV), and a capacity for hard work. His failure is explained as the result of his inability as an outsider to gain support within Prussia and his difficulty as a moderate in attracting either the liberals or the conservatives in Germany to his program. But, while the book is certainly adequate on Radowitz, it fails to provide much depth or perspective on the German question. The author's point of view is one of uncritical admiration for Radowitz, and, like the *kleindeutsch* historians of the past, he assumes that unification under Prussia was the best solution to the German problem. He gives Radowitz proper credit as an unheralded forerunner of the German Empire, but he fails to give him his due as a militarist, German chauvinist, and anti-Semite—the darker side of that tradition. While well organized and written in a clear style, the book has numerous minor errors, such as confusing the Austrian statesmen Friedrich and Leo Thun-Hohenstein.

ROY A. AUSTENSEN
Illinois State University

REINHARD SPREE. *Wachstumstrends und Konjunkturzyklen in der deutschen Wirtschaft von 1820 bis 1913: Quantitativer Rahmen für eine Konjunkturgeschichte des 19. Jahrhunderts*. Assisted by MICHAEL TYBUS. Göttingen:

Vandenhoeck and Ruprecht. 1978. Pp. 215. DM 26.

Reinhard Spree's book is a major contribution to German economic history. And, since recent political historiography has often drawn on business fluctuations for explanatory background Spree's findings will surely attract the curiosity of this wider audience as well.

The author wants to construct a quantitative framework for a history of business or growth cycles in nineteenth-century Germany. He uses eighteen time series with yearly data from 1820 to 1913, including variables on population growth, bankruptcies, the capital market, agricultural and industrial prices and production, and industrial productivity, profits, and investment, most of which bear on key industrial sectors like cotton, coal, and iron. In analyzing these time series both traditional and new methods are applied: Spree has chosen polynomial functions of the second to the fourth degree to fit the optimal time trend of each time series, with the residuals taken to detect peak and trough years. Their clustering in certain three-year periods allows Spree to identify business cycles of the economy as a whole. In order to solve the same problem through a different approach, an (American) National Bureau of Economic Research-type diffusion index is constructed, with each indication given the same weight. In a separate chapter Michael Tybus presents a spectral analysis of the variables. Thirteen out of the eighteen indicators reveal Juglar cycles between seven and nine years, some also show Kuznets (sixteen to twenty-five years) and Kitchin (three to five years) cycles, whereas Spiethoff's and Schumpeter's findings of long swings, *Wechselspannen* or Kondratieffs respectively, are rejected. Spree admits, however, that the existence of the "great depression" cannot be ruled out altogether, since years of expansion only narrowly outnumbered those of contraction. Although the author denies long swings the quality of a sound (empirically verified and theoretically deduced) concept, he nevertheless insinuates different stages in German economic growth: early industrialization, take-off (like Rostow from 1850 to the early 1870s), and high industrialization. These growth phases are often referred to when explaining structural breaks in the cyclical behavior of the variables.

Spree also tries to determine causal relationships between the variables by correlating the residuals with different lags. There is no indication of a correlation between population growth and the business cycle, for example, nor is there any evidence of a relationship between agricultural cycles and those in nonagricultural output. As Spree himself suggests, it might complement his study to analyze in

greater detail cyclical fluctuations within certain sectors and their relevance to the economy as a whole as well as to scrutinize the degree of market integration, especially before the 1850s.

In order to apply aggregate indicators, the regional business fluctuations ought to bear sufficient congruity. But during half of the period in question "Germany" was not a political entity and was just on its way to becoming an economically integrated state. On this ground two indicators may be questioned. In the first place, Scottish pig iron prices reflected British rather than German business cycles. Thus, Spree cannot trace any response of German iron production to the movements of these world market prices until well into the 1840s (pp. 132 ff.). In the second place, this independence was due to the structural change in German pig iron production (an indicator also used)—the transition from charcoal to coke pig iron. An aggregate measurement, however, veils the differences in the demand-supply conditions for these two different goods produced in different regions. Hence, these indicators are hardly feasible for tracing overall German business fluctuations before 1850.

It is a pity that in this book Spree refrains from tying himself down to a clear-cut, peak-to-peak and trough-to-trough dating of the detected cycles. On this the reader has to see Spree's recent article in *Geschichte und Gesellschaft* (5 [1979]: 237). But, in spite of these minor objections, Spree's book marks an important step toward a better understanding of nineteenth-century German economic growth and its fluctuations.

RAINER FREMDLING
University of Münster

WOLFGANG HOTH. *Die Industrialisierung einer rheinischen Gewerbestadt—Dargestellt am Beispiel Wuppertal*. (Schriften zur Rheinisch-Westfälischen Wirtschaftsgeschichte, number 28). Cologne: Rheinisch-Westfälischen Wirtschaftsarchiv zu Köln. 1975. Pp. 283.

This book is a useful addition to the still-growing literature on German industrialization. Like much of that literature it is a local study, focusing on the industrialization of the Wupper valley in the Rhineland and deals mainly with the nineteenth century. Its usefulness derives from the importance of its subject and the well-organized presentation of much information about that subject. The Wupper valley—the *Wuppertal*—is historically important as one of the earliest centers of industrial growth in Germany. It had already achieved considerable renown as a center of textile exports by the beginning of the nineteenth century, and its continued growth

had spin-off effects elsewhere in Germany, especially in the neighboring Ruhr area. It is not too far-fetched to see the Ruhr's development in part as a function of demands (for coal, for example) and capital and entrepreneurial skills originating in the more advanced *Wuppertal*. As Wolfgang Hoth reminds us, this was a good-sized region: for example, the twin cities of Elberfeld and Barmen, which comprised most of the region, had in 1858 a population of around 100,000, in 1880 of 190,000, and in 1900 of roughly 300,000 (making it at that last date the eighth largest urban center in Germany). Its development, therefore, was of some consequence and deserves more attention in discussions of German industrialization than it has generally received (Wolfgang Köllmann's fine book on Barmen, *Sozialgeschichte der Stadt Barmen*, scarcely mentioned Elberfeld).

Readers interested in the *Wuppertal* as a case study in industrialization will be grateful to Hoth for his well-ordered presentation of relevant facts. The book has two parts. Part 1 discusses prerequisites or essential factors of industrialization such as transportation facilities, supplies of labor and capital, entrepreneurial leadership, and so on. Hoth goes down his list, identifying times and/or instances in which a particular factor became scarce and/or critical and then discusses the Wupper response. Part 2 then presents a more chronological narrative of *Wuppertal* industrialization. This section offers a 5-stage periodization based mainly on the criteria, degree of mechanization of production and sectoral distribution of the labor force. It begins with the transition from preindustrial to early industrial conditions (1780–1820), ends with "mature industrialization" or *Hochindustrialisierung* (1882–1907), and thus appears to reject—for the *Wuppertal*'s industrialization—the watershed notion of development attached to concepts like "industrial revolution" or "take-off." Perhaps aggregate interpretations of Germany's industrialization that stress discontinuity require revision, after all.

A "useful" book is not necessarily a flawless one. I should like to mention two weaknesses that ought to interest American readers in particular. First, it is a rather parochial book, containing virtually no comparative references to analyses of industrialization in other countries or, indeed, to non-German sources at all. It fails, for example, to mention the important and accessible work on *Wuppertal* by the late Herbert Kisch, who did draw parallels between industrialization in the Wupper valley and industrialization elsewhere in the world. Second, it has no theoretical framework—neither of the general Marxian nor the specific neoclassical variety—and resorts at best to *ad hoc* theorizing. This limitation creates difficulties for readers who want to draw

theoretically oriented conclusions. The treatment of labor supply, for example, moves quickly from an interpretation stressing the positive effects of abundance to one stressing the virtues of scarcity (for labor-saving technical progress) without providing any systematic evidence that enables readers to decide the issue themselves (pp. 79–81). The same thing is true in the discussion of migration (pp. 72–73). It is quite likely that no one theory will do justice to these facts, but, by allowing the facts to dominate, Hoth makes generalization difficult and the reader's job slow going.

RICHARD H. TILLY
University of Münster

ROLF WILHELM. *Das Verhältnis der süddeutschen Staaten zum Norddeutschen Bund (1867–1870)*. (Historische Studien, number 431.) Husum: Matthiesen Verlag. 1978. Pp. 192. DM 48.

In view of German historiography's turn from great personalities and great-power politics to domestic issues, Rolf Wilhelm's book at first glance may appear out of step if not old-fashioned. Is there still more to be said about German unification between 1867 and 1870? Indeed, the topics on which Wilhelm focuses are familiar: military reorganization in the south along Prussian lines after 1866, the reform of the *Zollverein* in 1867, and the project of a South German Confederation. Wilhelm's argument and the material he marshals, however, make a significant contribution to the question of whether a German national state under Prussia was primarily a consequence of Bismarck's last war. He extends Otto Becker's thesis, found in his posthumous *Bismarcks Ringen um Deutschlands Gestaltung* (1958), that German unification was well under way before the Franco-Prussian War. Wilhelm sees Prussia's relationship with the south as important as that with the great powers.

Wilhelm devotes more than one-third of the book to the south's military reorganization following the Peace of Prague. This reorganization occurred most energetically in Baden. The first state to seek a military convention with Prussia, Baden in 1867 made the Prussian military plenipotentiary in Karlsruhe its minister of war and in the following year appointed Prussian officers to key army positions. Reforms in Bavaria and Württemberg, although not going as far in undermining their states' rights, followed similar lines.

Political attempts to carry out the promise in Article IV of the Peace of Prague of a South German Confederation are also well known, but Wilhelm adds more weight to his argument through the use of south German archival material; he includes twelve pages of key documents. Popular opposition,

whether democratic or ultramontane, to Prussian domination was very strong—it swept the field in the elections to the newly formed customs union parliament in 1868. This fact, however, also explains the reluctance of the mutually jealous southern dynasties to establish a united southern parliament under the control of oppositional democrats and clericals. Thus, the Bavarian Prime Minister Hohenlohe's projected South German Confederation was, as the Württemberg Varnbüler realized, impossible within the framework of monarchy.

The force of military assimilation, southern pressures for economic unity (which Wilhelm correctly notes have not yet been properly studied), and the internal contradictions of any monarchical "third Germany" were moving southern Germany slowly into the arms of the North German Confederation. It is in this context that Wilhelm sets Bismarck's diplomacy of not forcing "unripe fruit" and his *Kaiserplan*. The latter, designed to outflank the nationalists and the particularists, offered both unity and an apparent return to the old Reich, not through Prussian conquest but by Hohenzollern adoption of a German imperial crown. When this project ran into opposition, especially from London and Munich, the vacancy on the Spanish throne provided an opportunity for Bismarck to nudge the inevitable unification along. Thus, Wilhelm ends on two points: after 1866 there was, in the long run, no alternative for the southern states to a national state along the lines Bismarck provided; and the controversy over the Spanish-throne candidature needs to be re-examined in this light.

Not everyone will be persuaded; nor are these ideas new. Wilhelm plays down opposition within the south, and his Bismarck is more patient, even-handed, and pacific than that of most historians. But this book shows that there is still life in the old questions of diplomatic and political history.

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MOSCHE ZIMMERMANN. *Hamburgischer Patriotismus und deutscher Nationalismus: Die Emanzipation der Juden in Hamburg, 1830–1865*. (Hamburger Beiträge zur Geschichte der Deutschen Juden, number 6.) Hamburg: Hans Christians Verlag. 1979. Pp. 266.

It is difficult to disagree with Mosche Zimmermann's insistence that Jewish history ought to be considered as "an important, integral aspect of the historical totality . . . that is closely linked to all areas of history and not isolated from history in general" (p. 9); and, on the whole, he is to be commended for his efforts to locate the experience of

Hamburg's Jews within both an urban and a national context for the middle third of the nineteenth century. This period witnessed profoundly important developments at three levels: legal emancipation for Hamburg's large and prosperous Jewish community, reform of the city's constitution, and essential determination of the future shape of a united Germany.

The author states his basic question as follows: "How relevant were considerations and activities related to German unification on the one hand and local patriotism and local interests on the other hand for the relationship between Jews and non-Jews in Hamburg?" (p. 11). He answers this question in two ways. In the opening chapters he emphasizes repeatedly that both spokesmen for and opponents of Jewish emancipation generally argued in terms of the well-being of the city; the former contended that economic discrimination deprived Hamburg of the best energies of some of its most capable inhabitants, whereas the latter sought to defend the interests of the city's lower middle classes. In the middle and later parts of the book, Zimmermann focuses less on the reasons men gave for their views than on the means thought necessary to achieve the ends desired by Jewish leaders. Could emancipation be carried out through gradual reform in Hamburg, or would it require revolutionary changes at the national level? Gabriel Riesser, the most characteristic individual treated in the book, reflected the views of many other Jewish liberals when he turned in despair to national politics during the 1840s, only to retreat once more to the local level after 1849, when legal equality was largely achieved within the city. The completion of Jewish emancipation in the 1860s subsequently confirmed the Jews' patriotic support for their Hanseatic homeland.

Although the author successfully makes his case, one may still ask how important a case it is. The central question has the advantage of opening up a wide frame of reference, but it does not lead to conclusions that strike this reviewer as particularly surprising. It is also regrettable that he does not say anything about localism versus nationalism among Jews in other parts of Germany. Finally, although this is not really a criticism of Zimmermann's book, he does not offer a general account of the Jewish experience in Hamburg itself. For that, one must still turn to the works of Helga Krohn: *Die Juden in Hamburg, 1800-1850: Ihre soziale, kulturelle, und politische Entwicklung während der Emanzipationszeit* (1967); and *Die Juden in Hamburg: Die politische, soziale, und kulturelle Entwicklung einer jüdischen Grossstadtgemeinde nach der Emanzipation, 1848-1918* (1974).

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MARION A. KAPLAN. *The Jewish Feminist Movement in Germany: The Campaigns of the Jüdischer Frauenbund, 1904-1938*. (Contributions in Women's Studies, number 8.) Westport, Conn.: Greenwood Press. 1979. Pp. xi, viii, 229. \$17.50.

The Jewish Women's League (JFB), founded in Germany in 1904 and numbering fifty thousand women by 1925, aimed to maintain the traditions of the Jewish religion and the solidarity of the Jewish community and concentrated on charitable activities among German Jews. Although it belonged to the umbrella organization of German feminism, the *Bund Deutscher Frauenvereine* (BDF), the JFB, as Marion A. Kaplan notes, was isolated socially from German feminism because of its emphasis on "Jewish-mindedness" (p. 21). "The self-conscious feminism of the BDF . . . discomfited Jewish women" (p. 83), and, while the BDF concentrated on "improved educational, career and political opportunities for women," the JFB "bore a comfortable resemblance to traditional Jewish charities" (p. 83). In view of this it seems something of a misnomer to describe the JFB as a "feminist organization" (p. 136) and to talk of "its normally keen feminist analysis" (p. 201). JFB leaders insisted that "we are not the so-called emancipated women" (p. 75); and, if they campaigned for participation in the decision-making bodies of Jewish welfare organizations and encouraged women to gain an independent voice in Jewish life, they did not do so out of any conscious intention to use conventional female activities as a means of gaining equality, as Kaplan realizes (p. 75). Feminism, at its most minimal definition, involved some kind of ideological commitment to female equality (however conceived), and this the JFB did not have. Kaplan conveys the impression that it actively endorsed wider feminist goals such as the vote (pp. 58, 76), but she presents no evidence that it ever demanded anything more than women's suffrage on local and Jewish welfare and educational bodies. Ultimately, she is forced to admit that "the JFB sometimes lost track of its feminism while it absorbed itself in social work" (p. 74). It would have been less confusing to the reader had she dropped the term "feminist" altogether.

Despite this rather strained attempt to give the JFB feminist credentials, however, Kaplan's book, based mainly on the JFB's periodical, is clearly written, well organized, honest, and perceptive. Kaplan is good on Jewish women's ambivalent attitude to assimilation into German society and on the problems they encountered within the Jewish community. She is aware of the JFB's overwhelmingly middle-class composition and has some pertinent criticisms of its campaign to solve the "servant problem" through "home economics training" (pp. 179-81). On the JFB's major campaign against

"white slavery," however, I cannot help feeling that she has taken too much of the evidence at face value and that she has missed an important opportunity in failing to explore the psychological implications of the fact that the leading JFB campaigner in this respect, Bertha Pappenheim, first gained notoriety in another sphere as Freud's "Anna O." Her account of German feminism in general, moreover, is open to question: she is wrong, for example, in portraying the JFB as the most important religious organization in the BDF (p. 152)—the Protestant women's society was far more significant—and in saying that the female suffrage society neglected the vote in its early days (p. 64). Finally, the book's abrupt ending is unfortunate, to say the least.

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FRITZ FISCHER. *Bündnis der Eliten: Zur Kontinuität der Machtstrukturen in Deutschland, 1871–1945*. Düsseldorf: Droste Verlag. 1979. Pp. 112. DM 19.80.

This book is an expanded version of a lecture that Fritz Fischer delivered to the German Historical Association in October 1978. The title conveys its central argument. Fischer believes that there is an essential continuity in German "power structures" from 1871 to 1945, which was provided by a conservative alliance of "agrarian-aristocratic and industrial-bourgeois" power elites. Although the form of this alliance changed, its goals did not: a defense of power and privilege against the challenges of democracy and socialism. In times of crisis, this position required policies of repression at home and aggression abroad that ultimately culminated in the Nazi dictatorship's drive for world power.

Those familiar with Fischer's work will find nothing new in this book, but it does give a concise and vivid summary of the themes that he and his students have been developing for almost two decades. As such, it gives us an opportunity to review the accomplishments and shortcomings of the "Fischer school." First, I think that it is clear Fischer has succeeded in adding to our knowledge about the role of elites in the making of foreign and, to a lesser degree, domestic policy during the first half of the twentieth century. Second, he has managed to cast doubt on the sharpness of the distinction that some historians have tried to make between National Socialism and the main lines of German historical development. Once again, this achievement is much clearer in the area of foreign than in domestic affairs. Fischer has not, however, been able to demonstrate that his concept of continuity is the best organizing principle with which to understand the period between the formation of the Second Reich and the destruction of the Third.

Before we can accept the concept of continuity as Fischer defines it, I think we must know a good deal more about three issues. First, we need a precise and comprehensive analysis of the origins, education, occupations, and values of German elites from 1871 to 1945. We have gone as far as we can go with vague categories such as "agrarian-aristocratic" and "industrial-bourgeois," which may suggest some similarities over time but which also can mask some fundamental differences. Second, we need a much finer-grained picture of the political system. We now know quite a bit about some individuals and institutions, but we are a long way from understanding the changing nature and location of political power in the system as a whole. In this endeavor, notions like "power structures" and "power elites" do not help very much if they are used as slogans rather than as heuristic devices. Finally, we need some comparative studies in order to see if the power of conservative elites was in fact greater in Germany than in most other Western nations. In my opinion, a rather more plausible case for "continuity" can be made with regard to Britain, France, and Italy than it can for Germany.

Until we have a better idea about how to deal with these issues, the concept of continuity in German history must remain no more than a potentially useful hypothesis. It should not be allowed to ascend into the realm of accepted dogma, where it would obscure questions rather than illuminate them.

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THOMAS E. WILLEY. *Back to Kant: The Revival of Kantianism in German Social and Historical Thought, 1860–1914*. Detroit: Wayne State University Press. 1978. Pp. 231. \$17.95.

Neo-Kantianism was the major philosophical school in Germany from 1871 to 1914, yet the importance of this movement has slipped from historical memory. Two world wars, the Great Depression, the rise and fall of National Socialism, and, finally, the post-1945 conflict between East and West have made the lives and concerns of pre-1914 German intellectuals appear remote and irrelevant to our time. The significance for us of the movement back to Kant is that several generations of German liberals, in the midst of rapid economic and social change, failed to convince their compatriots of the value of individual liberty. Thomas E. Willey's study of Neo-Kantianism from the revolutions of 1848 to the Weimar Republic examines the fate of this neglected philosophical school.

Starting with Kant's credo, "Have the courage to use your own reason," Neo-Kantians took Kant's

concept of the autonomy of reason and united it with a view that the purpose of social existence is to encourage "the freedom of the individual to enhance his moral worth under laws hypothetically of his own making" (p. 23). By examining the Neo-Kantian applications of these principles to contemporary political and philosophical problems, Willey argues that he can see in these activities "the tentative and unsuccessful efforts of a segment of the upper bourgeoisie to make peace with the proletariat and to retain an attitude of cultural community with the West" (p. 23).

Willey divides adherents of the Neo-Kantian movement into two main groups. The early participants like Rudolf Lotze, Kuno Fischer, Eduard Zeller, Otto Liebmann, and Friedrich Lange revived Kant's ideas by rejecting metaphysics and forging an epistemological approach to philosophical problems. Lange closed the early phase by writing a definitive critique of materialism in 1866 and attempting to put Kantian ethics in the service of socialism. Willey gives Lange a special place in the development of Neo-Kantianism as the man who momentarily closed "the dangerous gap between thought and social reality" (p. 101).

The second phase of Neo-Kantianism, according to Willey, encompasses the works of Herman Cohen, Paul Natorp, Rudolf Stammler, Wilhelm Windelband, and Heinrich Rickert. These men examined problems of logic and values and methodological issues in light of the difference between natural and human sciences; yet their works turned increasingly to social and political questions. When examining the consequences of Neo-Kantianism in the Weimar Republic through the works of Ernst Troeltsch, Max Weber, and Ernst Cassirer, Willey attempts to convey the tragedy of Neo-Kantian liberalism: its inability to establish a viable link with the left and its failure to convince all Germans of the value of individual liberty.

Why did the Neo-Kantians fail to make a lasting mark on German society? Was it the result of their own philosophical outlook or the fault of forces beyond their control? The author believes that the Neo-Kantians had the right ideas but that the rest of the society let them down. Willey does not prove his case. He fails to explore the possibility that the individualistic orientation of Kantian philosophy would be repugnant to socialists who believed in the power of collective action; nor does he question that the abstract level of Neo-Kantian discourse would be incomprehensible and unacceptable to most Germans.

Lange's ability to take the fundamental insight of Kantian epistemology, the self-determining power of human reason, and apply it to the practical needs of the socialist movement is undeniable. Yet few liberals followed where Lange dared to tread.

Willey ignores this problem. In chapter 5, Willey emphatically denies that the Marburg failure to make allies on the left is the result of a flight from reality into philosophical abstractions (p. 103). The author proceeds to describe their works, however, in a manner that contradicts his generalization about them. Cohen made Neo-Kantian epistemology "a purely formal study of conceptualization" (p. 109), and Stammler's analysis of law led him to withdraw all empirical content from law in order to find an abstract, universal standard (p. 127). Clearly, some Neo-Kantians were trying to show that a priori laws of human reason were the basis of human actions. In pursuing this goal they may have demonstrated the self-determining character of human reason in theory, but the practical result was political irrelevancy. No one outside the movement was listening.

Willey's study is bold in its conception but faulty in its execution. The author tries to cover too much territory with too little time devoted to each man's philosophy. Consequently, the reader is given a few general ideas on each person but is left seeking the connection between these men's philosophies and the book's main argument. The author ascribes to the Neo-Kantian movement a liberal democratic bias that tried to go beyond the limits of German philosophy and middle-class politics. In fact, Willey has presented us a group portrait of individuals who expressed rather than transcended those limits.

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KARL HEINRICH POHL. *Weimars Wirtschaft und die Aussenpolitik der Republik, 1924-1926: Vom Dawes-Plan zum Internationalen Eisenpakt*. Düsseldorf: Droste Verlag, 1979. Pp. 452. DM 48.

Karl Heinrich Pohl believes that historians of German foreign policy in the mid-twenties have concentrated too heavily on the controversial and well-documented activity of Gustav Stresemann and too little on the role of the German iron and steel industry. His study focuses mainly on official trade treaty and private steel cartel negotiations between Germany and France, beginning after the London Conference of 1924 and ending with the private International Steel Pact of September 1926 and the final Franco-German trade treaty of August 1927. He relates these negotiations to the marketing problems of the French and German steel producers and to the conflict between the interest of German steel-makers in higher steel prices and that of the German machine industry in lower steel costs and better access to French markets. He also maintains that the Ruhr industrialists, by gaining control of their domestic and foreign markets, enhanced their ca-

capacity to pursue repressive social policies and reactionary political maneuvers at home.

At the start, Pohl writes that he regards the regaining of Germany sovereignty in trade policy (January 1925), the December 1924 and May 1926 agreements between the German steel and machine industries, and the International Steel Pact as "pre-suppositions or else pathfinders for 'Locarno' and 'Geneva' (the diplomatic high points) and as decisive events of German foreign policy" (p. 5). (Pohl's concepts are often loose, *Aussenpolitik* attaining Heideggerian obscurity on page 278.) Pohl does show that the Ruhr industrialists could veto a government initiative counter to their interests, the 1925 Saar agreement. But he does not show that industry cleared the way for, let alone influenced, Stresemann's proposals. Neither does he demonstrate (as Hans-Ulrich Wehler has done with grain tariffs in the 1880s) that interest-group programs indirectly shaped diplomatic relations. Actually, Pohl plays down the evidence in his sources that the chief Ruhr negotiator, Fritz Thyssen, wanted to use the strong position of the German steel industry to force a French evacuation of the Rhineland and other political concessions; this evidence is awkward for Pohl's thesis of dominance by heavy industry, since Thyssen's policy was not adopted. Pohl stresses the greed and willfulness of the steelmakers and seems to understate their problem of overproduction, their ultimate subordination to the political judgment of the Foreign Ministry, and their nationalism. This last was strong, even if it often served, and sometimes yielded before, their own advantage.

Pohl has done very extensive research, using forty-seven German-language archival collections. French public and private archives, however, might have added information, insight, and perspective. Pohl's bibliography is weak in non-German studies; notable omissions are Charles S. Maier's *Recasting Bourgeois Europe* (1975) and F. G. Stambrook's "Das Kind" article (*Central European History*, 1 [1968]: 233-63). The relation of the cartel negotiations to Stresemann's policies deserves another study, less parochial in its sources and more precise in its concepts, a study that examines its subject as an open problem and arrives at conclusions developed from relevant evidence by persuasive argument.

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JÜRGEN ARETZ. *Katholische Arbeiterbewegung und Nationalsozialismus: Der Verband katholischer Arbeiter- und Knappenvereine Westdeutschlands, 1923-1945*. (Veröffentlichungen der Kommission für Zeitgeschichte. Series B, Forschungen, number 25.) Mainz:

Matthias Grünewald Verlag. 1978. Pp. xxix, 252. DM 56.

The small-print subtitle of this monograph, *The Association of Catholic Workers' and Miners' Clubs in Western Germany, 1923-1945*, is a much better guide to its contents than the title. Historians hoping for that long-needed study of the last years of the Christian Trade Unions must remain patient. After giving a sketchy summary of its early development, Jürgen Aretz charts in exhaustive detail the policies of the association's leadership after 1930. The four main leaders, Joost, Müller, Letterhaus, and Gross were strongly anti-Nazi from an early date: one of them survived the concentration camps, one died in a prison hospital, and two were executed after July 20, 1944. Their organization, by far the largest in the Reich Association, covered the ten dioceses of northwest Germany; in 1930 the 192,000 members were organized in some 2,000 clubs and were catered for by 81 functionaries and by a weekly newspaper that sold over 150,000 copies. The internal structure of the association was authoritarian. It was a powerful organization that commanded a great deal of loyalty from its (gradually aging) male membership. (On the position of female Catholic workers the author says nothing.)

The popularity of the clubs is not adequately explained in this study. The services offered to members are only touched upon briefly, the nature of club life is barely mentioned, and relations with the Christian Trade Unions remain obscure. But popular they were: pilgrimages of disguised political protest attracted 200,000 participants in the latter part of 1934, and membership held up well until 1936 despite Nazi pressures. The clubs escaped destruction in 1933 because they were inscribed as religious rather than social or economic organizations, and they could plausibly claim protection under the concordat. The German Labor Front denied this claim and sought to prohibit dual membership. Against the background of recurrent local campaigns of intimidation, the Catholic hierarchy put the association's case (not always firmly or with enthusiasm) to the Reich government, which simply played for time. These protracted nonnegotiations are described well. That the undermining of the clubs in seven dioceses was a slow and piecemeal affair was due in part to the tactical skill and pertinacity of the association's leaders and to the willingness of members to risk losing their jobs or their supplementary pension rights. In 1938, however, the newspaper was permanently banned, and in 1939 the dissolution of the whole association appears only to have been forestalled by the intervention of General von Hammerstein-Equord. During the war activities and membership declined still

further, and the leaders engaged in political resistance.

This study rests on much newly available source material, and the narrative of the leadership's defensive strategies is potentially of general interest to students of the Third Reich. But Aretz's approach is remorselessly monographic; he leaves his readers to draw all their own conclusions concerning the wider significance of his findings, even within the context of the *Kirchenkampf*. It does not become fully clear why the persecution of the association was not more severe—the dissolution of all clubs in the Münster area in 1935 showed that the regime could risk a confrontation without causing major public disturbances. This revised version of a Bonn thesis tends toward homage to the leaders of the association rather than toward analysis. It is, however, anything but a popularizing celebration of these unusual men—the prose is leaden and ungainly, often with obscure allusions and three footnotes per sentence.

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WILLIAM E. GRIFFITH. *The Ostpolitik of the Federal Republic of Germany*. (Studies in Communism, Revisionism, and Revolution, number 24.) Cambridge: MIT Press. 1978. Pp. 325. \$25.00.

This is a remarkable and in large part authoritative treatment of the complex relationships of the Federal Republic of Germany with the Soviet Union and various Eastern European countries. Beginning with some historical antecedents, the author carefully reconstructs the Eastern policies of successive West German governments by contrasting lists of minimal and marginal goals with the possibilities and achievements of each of the governments involved. There is probably no better secondary treatment available today than William E. Griffith's analysis of the varied motives of different Eastern European governments at the time of the Czechoslovak invasion of 1968 or of the *Ostpolitik* initiatives of Schroeder, Kiesinger, and Brandt. Even the impact of China on the polycentric perspectives of *Ostpolitik* and on the "globalization" of European politics receives its well-considered due. Against this background, West German *Ostpolitik* measures evolve with a certain logical inevitability rather than as the creative, or even subversive, schemes of certain men or as shortsighted concessions to the expanding colossus of Soviet power. The author concludes, quite rightly in this reviewer's opinion, that the *Ostpolitik* of the early 1970s on balance produced substantial benefits for the Federal Republic

of Germany, West Berlin, the West in general, the Soviet Union, and Eastern Europe, though perhaps least of all for the East German DDR.

In the midst of a welter of discursive details, Griffith often succeeds in giving the reader revealing insights into the nature of German politics such as in his characterization of the new German left: "the latest example of the recurrent German revolt against Western bourgeois, affluent, materialistic society, as opposed to ascetic, egalitarian, Prussian, romantic virtues . . ." (p. 163). His description of how the Czechoslovak invasion actually served to clear the decks for the rapprochement to follow (pp. 160–61), his multilingual analysis of the text of the Renunciation-of-Force Treaty between Bonn and Moscow (pp. 191–93), and his perceptive account of the decline and fall of Walter Ulbricht (pp. 201–06) are among the high points of this book.

On the debit side, the author has not made it easy for readers to find their way through the endless turns and details of West German foreign politics of the 1960s. Even specialists may well learn more here than they might care to know. Only rarely is there a summary or other guidepost for the reader. The copyeditors, too, have failed to use their blue pencils and have let innumerable typographical errors and the like slip by, particularly in the last chapters. Finally, the author's strength lies obviously more in the analysis of complex international relationships than in the description of domestic structures and conflicts. The tension between the democratic politics of parties, elections, and public opinion polls and the international interaction of policy making and intellectual elites is not always resolved in this book.

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BRIGITTE HAMANN. *Rudolf: Kronprinz und Rebell*. Vienna: Amalthea. 1978. Pp. 535. S 298.

The life of Crown Prince Rudolf von Habsburg symbolizes the hopes and disillusionments of Austrian liberalism between 1858 and 1889. His mysterious suicide at Mayerling and the chance that he might have been the savior of liberal Austria continue to stimulate a larger literature than any of his actual achievements would justify.

Those already familiar with the literature on Rudolf should not expect major revelations from Brigitte Hamann's new biography, but it is well written and displays a thorough knowledge of the sources, many of which have become available only since Oskar von Mitis's standard biography of 1928. This shortened version of a dissertation, completed under Adam Wandruszka in 1978, remains longer

and more detailed than necessary; but Hamann does present a full picture of Rudolf's thought and attitudes. She portrays Rudolf's remarkable education, his love of science, his devotion to his military duties, his secret career as a liberal journalist, and his political impotence as heir apparent. Her account succeeds not only as a sympathetic portrayal of a man's fate but also as an illuminating perspective on the decline of liberal values in the 1880s. Her argument builds persuasively and culminates in the inevitable preoccupation with the still unexplained double suicide.

Although Hamann rejects the theory of Rudolf's involvement in a Hungarian conspiracy against Franz Joseph, politics bulks large in her multicausal portrayal of Rudolf's state of mind before the suicide. Franz Joseph seemed unlikely ever to abdicate or to allow his son into circles of power and policy. Moreover, Rudolf was depressed by recent criticism of him in the anti-Semitic press and by the repercussions of his own sometimes dubious political activities. Finally, Hamann emphasizes the accession of his *bête noire*, Wilhelm II, to the German throne and the likelihood of a Franco-German war in the wake of Boulanger's electoral successes. Rudolf was convinced that war would mean the crushing of Austria by Russia and the end of his dream of a liberal-Slav alliance, including France, Austria, Russia, and perhaps even England.

Hamann seems to feel, however, that personal considerations weighed more heavily in Rudolf's decision to commit suicide. She is no more inclined than von Mitis to believe that Rudolf was in love with Mary Vetsera, but Hamann emphasizes Rudolf's gonorrhea and rapid physical decline, his loveless marriage, his father's denial of a divorce, Mary's pregnancy, and her adolescent encouragement of his suicidal fantasies. Apparently Rudolf was crushed by external factors too overwhelming to sort out, but a more psychological account would certainly have been justified. Rudolf's bad personal relationships with almost everyone in his family and in the court deserve more thoughtful analysis, and Hamann might have done more to make sense of his education, his emotional and religious development, and the tensions between the aristocratic and bourgeois components of his personality. All the available facts are here, but one can hardly avoid wishing that Hamann had attempted something more of a psychobiography.

Nevertheless, Hamann's book is now the most complete and scholarly account of Rudolf's life. The only justification for another biography would be firmer knowledge about the motives for Rudolf's suicide or the application of less traditional methodologies.

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STEFAN MALFER. *Wien und Rom nach dem Ersten Weltkrieg: Österreichisch-italienische Beziehungen, 1919-1923.* (Veröffentlichungen der Kommission für Neuere Geschichte Österreichs, number 66.) Vienna: Hermann Böhlau Nachf. 1978. Pp. 186. DM 58.

Wien und Rom nach dem Ersten Weltkrieg is a concise and remarkably objective account of a brief but critical period in Austro-Italian relations. The author's thesis is that the traditional enmity between the two countries need not have continued after the First World War. Stefan Malfer's detailed study of the period from the signing of the Treaty of St. Germain through Mussolini's first year in power describes the opportunity for good relations that the various governments on both sides—prior to Mussolini—were anxious to exploit.

The Italian people, or at any rate the democrats and socialists, were willing to forget the past and indeed pursued policies that were frequently beneficial to Austria. Even the South Tyrol was not an inevitable bone of contention; the democratic governments of Italy promised the area autonomy.

For diplomatically and economically isolated Austria, Italy was the only power that could help settle the many issues left unresolved by St. Germain. In the territorial disputes involving Carinthia and the Burgenland, its assistance was particularly valuable. Italy supported Austria's claim to most of southern Carinthia because it wanted to preserve its most direct rail link to friendly Hungary. Italy mediated a compromise for the Burgenland by which Hungary withdrew its irregular troops in exchange for Austrian agreement to a plebiscite in Ödenburg (Sopron). Although the Austrian public blamed Italy for the negative outcome of that questionable vote, Malfer points out that the Austrian political parties had already recognized the country's diplomatic weakness and the need for offering Hungary something tangible in return for receiving most of the disputed borderland.

Less helpful to Austria was Italy's role during the discussions leading to the Geneva Protocols and to the loan by the League of Nations in 1922. The attitude of the Italian delegation, however, was determined by its preference for an Austro-Italian customs union.

A reversal in the promising new Austro-Italian relationship came almost as soon as Mussolini took over as prime minister. The Duce aggressively pursued the aim of Italian nationalists and Fascists to make the Danube—especially Austria and Hungary—an Italian sphere of influence. For this policy good relations with Austria were obviously desirable. But even more important was the Fascists' goal of totalitarian centralization, which meant in part the Italianization of the South Tyrol. It was this policy that soured Austro-Italian relations. Malfer concedes, however, that the revival of

Anschluss sentiments in Austria helped aggravate growing Italian insecurity about the Brenner. By the end of 1923 all the issues that would lead to the deterioration of Austro-Italian relations were already apparent: Fascist radical denationalization, press wars, and angry parliamentary debates.

Wien und Rom nach dem Ersten Weltkrieg grew out of a dissertation written at the Historical Institute of the University of Vienna. It is based primarily on documents found in the Neue Politische Archiv of the Haus-, Hof-, und Staatsarchiv as well as on a wealth of secondary sources in German, Italian, and English. The author can be faulted only for neglecting to use Italian diplomatic documents and for providing too little background information, especially on Italy's role at the Paris Peace Conference.

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UMBERTO MAZZONE. *"El buon governo": Un progetto di riforma generale nella Firenze savonaroliana.* (Biblioteca di Storia Toscana Moderna e Contemporanea. Studi e Documenti, number 18.) Florence: Leo S. Olschki. 1978. Pp. xv, 217.

The *Riforma sancta et pretiosa*, published at Florence in 1497, was written by Domenico Cecchi, a modest artisan-shopkeeper. This text (transcribed in the appendix) has been known to historians of the Savonarolan years, but it has been neglected as a rude product of ignorance and partisanship. Umberto Mazzone, although he recognizes that Cecchi's treatise contained little of originality, attempts to salvage the reputation and historical value of the text. Taking as his point of departure the observation of Donald Weinstein that history of Savonarolan Florence devotes too much attention to personalities and far too little to society, Mazzone places the *Riforma* in context by discussing one-by-one the sixteen reforms proposed by Cecchi, setting them in the framework of the ongoing debates over those issues among supporters and opponents of Savonarola.

Mazzone begins by tracing Cecchi's family background and circumstances—a difficult task in which he achieves considerable success despite a scarcity of evidence—concluding that Cecchi's ardent pro-Savonarolan position and his interest in civic reform sprang from the fact that he was among that middle stratum of Florentine society that gained political participation as a result of Savonarola (though Cecchi himself seems never to have held office). Mazzone then advances to the heart of the book, the analysis of the particular proposals espoused by Cecchi. This painstaking analysis reveals that Cecchi was sensitive to the issues of his time: material issues like the equitable distribution of political offices and taxation, reform of dowry prac-

tices, encouragement of commercial activity, and military organization and moral issues like the elimination of sodomy (a catchword for a complex of sexual transgressions) and official corruption, establishment of a university, and expulsion of Jews. Cecchi's proposals also indicate that, although he was a Savonarolan, he was capable of criticizing and differing from Savonarola at points.

Mazzone's contextual analysis of the *Riforma* affords valuable insights into the issues current from 1494 to 1498. These insights, however, are limited by the author's method. He treats Cecchi's *Riforma* only contextually; he never discusses it as a whole to determine its textual logic. Also, the contextual logic he exposes is purely isomorphic with the text. One has no idea whether there were burning issues that Cecchi avoided or ignored. The piecemeal treatment of the *Riforma* means the reader never sees the forest for the trees. A more diversified approach might also have given Mazzone another perspective on an important issue—the *Riforma*'s silent reception in Florence. He argues that Cecchi's reforms were rendered moot by the waning of Savonarola's influence, but there is much more that could be said about the treatise itself. The unenthusiastic reception given the ideas of a typical member of the artisan class may provide a clue as to why Savonarola lost support and why there never was in Florence a *riforma sancta et pretiosa*.

THOMAS KUEHN

Reed College

GABRIELLA CIAMPI. *I Liberali moderati siciliani in esilio nel decennio di preparazione.* (Istituto di Storia Moderna, Storia, number 5.) Rome: Edizioni dell'Ateneo and Bizzarri. 1979. Pp. 235. L. 7,500.

The political experiences, ideas, and aspirations of the unsuccessful revolutionaries of 1848–49 have been an important theme of Italian historiography in the last twenty years. Like earlier works by Della Paruta, Scirocco, Ganci, and others, this book focuses on the lessons that the "founding fathers" of modern Italy learned from the failure of the revolution. Using the moderate liberal leaders of the Sicilian revolution as a test case, Gabriella Ciampi analyzes their political and intellectual development in the crucial decade before Italy's unification. Her analysis is based on a wide range of published sources and on a few, but important, manuscript collections, such as the Torre Arsa family papers.

In contrast to their left-wing colleagues, the most prominent Sicilian liberals were spared long prison terms or persecution by the governments of host countries. Important protagonists of the Sicilian revolution, such as Filippo Cordova, found a hospitable refuge in Piedmont-Sardinia. For that reason, Ciampi rightly focuses her attention on their en-

counter with Piedmontese culture and political institutions while she makes only passing references to Sicilian exiles abroad. Known in Piedmont-Sardinia as the men who had offered their island kingdom to a member of the House of Savoy, the Sicilian liberals settled in Turin or Genoa with relative ease and were quickly integrated in the society and economy of their host country. Nonetheless, nearly all of them remained reluctant exiles, eager for a second chance to lead the liberation of their beloved island from Bourbon rule.

Ciampi's sympathetic, crisply written account of their experiences has several merits. First, she clearly shows that the postrevolutionary debate between the Sicilian liberals and their democratic critics on the left did not hinge upon institutional issues (monarchy versus republic) or even upon issues of revolutionary strategy. Rather, it stemmed from profound ideological differences between the two groups concerning the goals of the Italian revolution. Reflecting differences of social background and aspirations, the liberals conceived those goals in primarily political terms while the democrats stressed the social and cultural dimensions of the national revolution. Second, Ciampi's careful reconstruction of individual and group experiences demonstrates that the Sicilian liberals' rapprochement with Cavour and other northern leaders in the late 1850s and their acceptance of his annexationist policy in 1860 came very slowly. Even as they joined their fellow Sicilian Giuseppe La Farina in the effort to unify Italy under the constitutional regime of Victor Emmanuel of Savoy, the Sicilian liberals had reservations.

Basically, Ciampi argues, the liberals remained faithful to the ideal of 1848: a Sicilian constitutional monarchy, separate from the hated Neapolitans but also separate from other Italian regions whose traditions and social structure were incompatible with theirs. Their preference for a separate Sicilian kingdom, however, did not reflect a provincial or narrow-minded outlook. Keen jurists like Cordova and eminent economists like Francesco Ferrara were fully capable of appreciating the institutions and civilization of other lands. But as Sicilians and as liberals they were convinced, in the light of the events of 1848-49, that only a separate kingdom under their own leadership would provide the appropriate conditions for a political revolution without social upheaval.

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RAFFAELE ROMANELLI. *L'Italia liberale (1861-1900)*. (Storia d'Italia dall'Unità alla Repubblica, number 2.) Bologna: Il Mulino. 1979. Pp. 534. L. 6,000.

Multivolume historical studies, whether dedicated to centuries of diverse matters or less extensive treatments of particular sectors of the national scene, seem to thrive and proliferate in Italy with amazing vitality. Although the need for or *raison d'être* of some of these productions is indeed questionable, others can provide a pleasant and rewarding surprise. The latter is the case with Raffaele Romanelli's volume on the first decades of the liberal kingdom in Italy.

Romanelli, a highly promising young scholar of the exciting postwar generation in Italy, has given us a most refreshing interpretive work on an already much-studied era, but he shows us throughout how much more reflection the period still deserves. Conceived with chronology only as a slightly abstract backdrop, this book is essentially topical. Naturally many of the topics are the same ones that have long been present in any account of the same timespan; but there is a great deal of new information, and some of the old issues are dealt with in some detail for the first time in a general history.

The theme of this book, obviously reflecting the author's political convictions, is that most of the problems, frustrations, and disappointments the new nation faced derived from a *révolution bourgeoise manquée*. Although there is nothing novel in that view, the topics Romanelli deals with and his mode of treatment contain a surprising number of stimulating and provocative ideas based on a wealth of reading and the fruits of recent research in Italy. The result is revisionism in its best manifestation. If Romanelli is clearly an economic determinist and if he has been much influenced by the Marxist tradition in recent Italian historiography, he nevertheless maintains a commendable independence of thought and objective detachment. In short, he demonstrates a certain maturity that is now appearing in the treatment of the heritage of the Risorgimento. If anything, he almost overstates the anachronistic nature of the thought and action of the generation of leaders who united Italy as well as the backwardness of the regions stitched into the boot, but he is always on well-documented ground. He is, thus, realistic, without being either tragic or dramatic, in his assessments of a truly pathetic and staggering set of obstacles for those who would make of Italy at least the smallest of the great powers.

Another praiseworthy characteristic of this book is its total abandonment of that mode of viewing Italian history in terms of the reigns of its great prime ministers. Cavour, Depretis, Crispi, and Giolitti are reduced to very human proportions in Romanelli's treatment. Also some of the oft-deprecated figures in this period get praise for their work in appropriately reasoned measure where they deserve it: Crispi, for his domestic reforms of the late

1880s; Sonnino, for his financial measures in the 1890s. The same is true in the evaluation of the Southern Question, shown to have been perpetuated as much by northern doctrinaires as by any native problems in the region. And the same can be said for a number of other topics that have become trite and jaded in the hands of others. In other words, what might appear to be just one more narrative covering the same old ground is, instead, a fascinating account with much new information in a compact, intelligent, and highly readable book. The style is refreshingly crisp, clear, and rich.

Whether the other volumes projected for this series can meet Romanelli's standard is yet to be seen, but the authors are very able. For the years prior to Romanelli's work, Franco della Paruta is to deal with the struggle for unification. Alberto Aquarone has the Giolittian period, Danilo Veneruso that of Fascism, and Pietro Scoppola Republican Italy. It is to be hoped that they will devote more space to foreign affairs, Romanelli's one area of excessive brevity.

The format of this book, a reasonably priced paperback, is fortunately modest—as unpretentious as its author. The contents are still a great bargain. Following the four hundred pages of text are a chronology (too sketchy) of the years 1861 to 1900, a complete listing of the governments with inclusive dates and the personnel heading the ministries, fifty pages of extremely useful and enlightening statistics, and, finally, an unusually rich fifty pages of bibliography. The convenience of these data for Italian readers is clear, but for foreigners it is even more useful since much of this information is difficult to find outside major research libraries.

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HUGO BUTLER. *Gaetano Salvemini und die italienische Politik vor dem Ersten Weltkrieg*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 50.) Tübingen: Max Niemeyer Verlag. 1978. Pp. xxiii, 498. DM 116.

Gaetano Salvemini (1873–1957) constantly pricked the conscience of the Italian nation. Hugo Butler's outstanding University of Zurich dissertation, based on an exhaustive examination of Salvemini's vast collected works as a political agitator and historian and the studies that he provoked, describes his thought and action to 1915.

Profoundly influenced by the poverty that he experienced and observed from his native Molfetta in Apulia and by positivism, which he encountered at the University of Florence, Salvemini maintained a primary interest in arousing and organizing the agricultural proletariat in the Mezzogiorno to seek a better life. He was impatient with speculative theo-

ries and an advocate of practical and concrete reforms such as free trade, more equitable taxation, the right to vote, a federal system of government, and school reform. One of his first practical successes came in 1901 when he helped organize the secondary school teachers into a union.

Salvemini was appointed professor of history at the University of Messina at 28. He lost his whole family in the earthquake of December 1908. In 1910–16 he was professor at Pisa and in 1916 became professor at Florence. He suffered defeats as a candidate for the Chamber of Deputies in 1910 and 1913 but was a member of the provincial council at Bari in 1914. Elected to parliament in 1919, he resigned at the end of 1920. He had been quite friendly to Mussolini in 1914, but he fled from Fascism in 1925 after helping to organize the underground publication *Non Mollare*. He taught at Harvard from 1934 to 1948 and returned to Florence in 1949.

Although a Socialist Party member from 1893 to 1911, Salvemini seems never to have been strongly attracted to Marxist theory and soon became one of the most severe critics of the Italian socialists. His reading of the works of Mazzini and Carlo Cattaneo in 1898–99 and the unrest and repression that he saw around him made him for a time a revolutionary republican and left him an advocate of federalism along the Swiss model. He first blamed the socialists for advocating moderation and calm during these troubled days and then for backing the Giolitti government in what he regarded as an unholy alliance with the feudal and city bourgeois elements in the South to keep the ignorant agrarian masses in subjection. Socialism, he charged, was sacrificing the agrarian proletariat for Giolitti's small favors to the industrial proletariat.

Having lost faith in all political parties, Salvemini established his own mouthpiece, *L'Unità*, in 1911 to agitate for major concrete reforms and to develop a new ruling class. His bitter opposition to Giolitti seemed to soften, but only briefly, when the latter began promoting manhood suffrage.

A severe critic of Italian imperialism, especially of the Libyan war, of the Triple Alliance, and of Italian foreign policy in general, Salvemini pushed for Italy's entry into war in 1915 on the side of the Triple Entente and volunteered for the army. A critical reappraisal of his views on foreign policy, based on a more complete examination of the scholarly studies now available, would have made this study even more valuable.

WILLIAM C. ASKEW
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FRANCA PIERONI BORTOLOTTI. *Femminismo e partiti politici in Italia, 1919–1926*. (La Questione Femminile,

number 18.) Rome: Editori Riuniti. 1978. Pp. 408. L. 4,800.

In *Studi Storici* in 1973, Franca Pieroni Bortolotti called for the kind of concrete and detailed research that would establish the "precise physiognomy" of the history of Italian women as distinct from the models already developed for other Western nations. She, herself, stands almost alone in fulfilling this prescription, having traced the development of the Italian women's movement in two earlier works, *Alle origini del movimento femminile in Italia, 1848-1892* (1964) and *Socialismo e questione femminile in Italia, 1892-1922* (1976). In her recently published third volume, *Femminismo e partiti politici in Italia, 1919-1926*, she analyzes the struggle between feminist and antifeminist organizations after World War I and until the repression of all non-Fascist women's groups by Mussolini's dictatorship in 1926.

Centering her book on the postwar debates concerning women's suffrage and divorce, Pieroni Bortolotti emphasizes that such nineteenth-century issues did not die with the war but re-emerged only to be co-opted by the Fascists until their power was great enough to deny civil and political rights to both sexes. The Chamber of Deputies, for example, overwhelmingly approved a bill granting political and administrative suffrage for all women, although the Senate failed to act on the issue. Feminist sentiment remained strong enough that in 1925 Mussolini supported a successful bill to allow a few categories of women, such as mothers of sons killed in war, to vote on the local level. Before these women went to the polls, however, Mussolini abolished the election of local officials, thus nullifying the very restricted concessions of his suffrage legislation.

Like much of Italian scholarship, Pieroni Bortolotti's book is shaped by her own explicit political views. A member of the Communist Party, she devotes over half the book to a detailed analysis of the position of those in the Socialist and Communist Parties as well as the Third International. She challenges the view held by many historians that the parties of the left have traditionally ignored the oppression of women to concentrate on class conflict; yet she is quick to criticize many of their members for sexism or insensitivity to the importance of civil and political rights for women. She admires those bourgeois feminists, often dismissed by their Marxist contemporaries, who kept a clear view of the importance of equal rights in the face of the Fascist challenge. Even when dealing with the Catholic and Fascist organizations, with which she is unsympathetic, she stresses the diversity and debate within these groups that have often been characterized as monolithic in their antifeminism.

Although Pieroni Bortolotti provides a wealth of new information, her manner of presentation hinders

understanding. Rather than being grouped around themes, her discussion often follows the chronological and internal order of the speeches, memoirs, and party publications that she consulted. This tedious and confusing organization obscures and weakens her major points. Furthermore, in her effort to place Italy in a comparative framework, she misrepresents the character of American feminism by basing her account almost exclusively on the memoirs of Elizabeth Gurley Flynn, a member of the I.W.W. Her portrayal of American feminists as predominantly socialist displays wishful thinking rather than the reality of a country that lacked the large and institutionalized Marxist parties of Italy.

Despite the defects in her book, Pieroni Bortolotti must be hailed as the pioneer of Italian women's history. It is to be hoped that a fourth volume will continue her narrative of the vicissitudes of Italian feminism after 1926.

MARY GIBSON
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DAVID D. ROBERTS. *The Syndicalist Tradition and Italian Fascism*. Chapel Hill: University of North Carolina Press. 1979. Pp. x, 410. \$22.50.

David Roberts's important and often controversial study traces the transformation of syndicalism from an ideology of proletarian renewal to "national syndicalism" that sought its social base among all producers, regardless of class. By tracing the development of thinkers like Agostino Lanzillo, A. O. Olivetti, and Sergio Panunzio, Roberts shows how national syndicalism merged with fascism and influenced Mussolini and several future Fascist leaders like Dino Grandi and Italo Balbo.

The author defies conventional wisdom about the purely reactionary character of fascism by linking left-wing fascism to the democratic heritage of Giuseppe Mazzini. In their attempts to grapple with a flawed liberal Italy, the syndicalists claimed part of the legacy of Mazzinian populism. Although Roberts is excessively hard on the leaders of liberal Italy, he reveals how defects in the economic and political system bred deep disillusionment. Bureaucratic overcentralization, economic parasitism, and political corruption led many young bourgeois intellectuals to identify with the proletariat and to seek an alternative society based on syndical association. This syndicalist faith in the proletariat had little in common with orthodox Marxism. These intellectuals, motivated by an intense need to find a mass base for their rejection of the existing political system, never established solid ties to the proletariat and during World War I drifted from proletarian syndicalism to national syndicalism of the productive classes.

Roberts stresses the sincerity and idealism of the syndicalists in their hopeless task of realizing through fascism a new conception of democratic mass participation based on corporative association. He devotes several perceptive chapters to the contrasts between syndicalist thought and the dominant nationalist orientation of fascism. Especially valuable is the final chapter dealing with the differences between fascism and nazism, due in part to left fascism's positive response to the challenge of modernization and to its ties with the democratic tradition.

On a few points, however, this book is misleading. Events like the Matteotti crisis are interpreted from a syndicalist perspective when the syndicalists were not decisive protagonists. The constant defeats of left fascism are explained away almost as though the author had a vested interest in reviving the corpse. Roberts is often excessively generous. It mattered little that Fascist labor leaders were sincere when the reality was that Fascist unions were totally unable to protect the workers, especially the large number of organized peasants, from the revenge of the employers. The failure of left fascism is inexplicable without dealing more fully with the opposition of powerful economic interest groups like the General Confederation of Industry. The author is simply not convincing in his efforts to show left fascism as a continuing possibility for the regime. Finally, the terms "left fascist" and "fascist populist" are used to refer to such diverse people (Bottai, Farinacci, Panunzio, Grandi) under such widely differing circumstances that they lose clarity.

Notwithstanding these reservations, this well-documented book will provoke a useful debate and will add a new dimension to our knowledge of Italian fascism.

ALEXANDER DE GRAND
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RICHARD CLOGG. *A Short History of Modern Greece*. New York: Cambridge University Press. 1979. Pp. viii, 241. Cloth \$26.00, paper \$8.95.

This book is part of a series of short histories of modern nations issued by Cambridge University Press and including already published surveys on Germany, Italy, France, Switzerland, England, Yugoslavia, Portugal, and Mexico. The author of this volume, Richard Clogg, is a specialist in Greek history, who has previously published studies on the pre-1820s and the post-1945 period. His narrative commences with a discussion of Byzantium after 1204 and concludes in 1978. His emphasis is on the modern era, with over half of the book (pp. 105–225) covering the years since 1913.

As the author recognizes, the major problem in preparing a short survey is the question of the material to be presented. It is difficult to fault the choices made here. The book emphasizes domestic political history and foreign policy, subjects on which a great deal of basic research has been done. Clogg regrets "having devoted less attention to . . . social, economic and cultural developments" (p. vii), but, in fact, he has included these aspects of Greek history when they are of crucial importance. For instance, he describes well the cultural background to the Greek Revolution of 1821 and the recurrent severe economic problems. The book, however, does not attempt to discuss life in the village and city as do, for instance, John Campbell and Philip Sherrard's *Modern Greece* (1968) and William H. McNeill's *Metamorphosis of Greece since World War II* (1978). The sections on international affairs deal primarily with Greek relations with the great powers and devote comparatively little attention to the country's neighbors. The conflict with Turkey over Cyprus is, however, fully described. Nevertheless, within the necessary limits on length, the author has succeeded in presenting the major issues facing the Greek people in these years.

This book should be extremely useful in classes on modern history and political science where a balanced, authoritative survey of Greek history is needed. This is certainly the best work of its kind available. It should also be of interest to anyone who would like to read a scholarly, well-written discussion of the background to the present events in the Mediterranean. One of the admirable aspects of this study is its careful treatment of the many controversial aspects of Greek politics, questions that are handled with reason and impartiality. The author, with wide experience in the field, has a sympathetic yet critical attitude toward his subject. It is to be hoped that this series will contain similar books on the history of other East European states.

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ISTVAN DEAK. *The Lawful Revolution: Louis Kossuth and the Hungarians, 1848–1849*. New York: Columbia University Press. 1979. Pp. xxi, 415. \$16.95.

This reconsideration of the Magyar struggle for independence, with its emphasis upon Louis Kossuth, tells its story logically and simply. Throughout, the author comes to grips with the legends that every romantic failure generates and with the controversies that national bias and economic determinism have stimulated. In the years 1971–73 Istvan Deak worked in the Hungarian National Archives to add to his analysis of the great amount of secondary material available.

Although Kossuth is its pivotal figure, the book contains admirable portraits of Lajos Batthyány, Arthur Görgey, György Klapka, Józef Bem, and Henryk Dembiński. Schwarzenberg and Windischgrätz receive shrewd appraisals, and Kossuth's wife, almost a Lady Macbeth in some accounts, emerges rather as a spinsterish and haughty person whose love for show gave rise to exaggerations of her influence over her husband.

Deak is sure that Kossuth fostered the "great crisis of the Empire" by his speech of March 3, 1848 (p. 62). The author argues that Kossuth was a dictator for only part of the period that ended with his exile and gives ample indication of the extent of his powers and of his encounters with Görgey. The challenges facing the Hungarian cabinet were the "Habsburg problem, the Croatian problem, the Jewish problem, the peasant problem, the worker problem, the radical problem, and the nationality problem" (p. 103). The author deals with the inevitable confusions of the time adroitly; his treatment of the military campaigns is condensed and yet quite adequate.

Deak does not hesitate to offer judgments, such as the following. The feudal diet of 1848 did more for social and economic reform than did the new House of Representatives, and it created voting provisions that were very progressive in the context of existing electoral arrangements elsewhere. Today it is impossible, as it was for Kossuth, to penetrate the motivation of the nobility, and there is no evidence that Kossuth wished to go further, after April 1848, to enhance the regime's standing with the lower classes. The country never suffered from a shortage of food and eventually it possessed a first-rate *honvéd* army. The government could not have created a system of self-government that would have ensured the predominance of the non-Magyar majority, but it might have worked out a compromise with some of the nationalities. The worst of its blunders was the attempt to defeat the Rumanians. Hungary did not become a republic on April 14, 1849, nor did the overblown Declaration of Independence force Nicholas I to intervene. The tsar worried over subversion everywhere, but he was only secondarily concerned with the chances of an explosion in Russian Poland. Moreover, his forces under Paskievich were rather lethargic, thanks to cholera and a rather surprising sympathy for the Magyars. Haynau and the Austrian soldiers were far more effective. The Hungarian nobles began to desert the cause when the deputies cut the ties with the Habsburgs; after May 1849, all classes realized that the war was lost and acted accordingly.

Excellent maps and illustrations complement this fast-paced and well-organized study.

WILLIAM A. JENKS

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JÁNOS DECSY. *Prime Minister Gyula Andrassy's Influence on Habsburg Foreign Policy: During the Franco-German War of 1870-1871*. (East European Monographs, number 52; Brooklyn College Studies on Society in Change, number 8.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1979. Pp. xviii, 177. \$12.00.

János Decsy's study of Count Gyula Andrassy's views of and influence on Habsburg foreign policy while he was the first prime minister of the resurrected Hungarian state from 1867 until 1871 is the latest of a wave of monographs on the makers of nineteenth-century Hungarian history. The wave began with George Barany's *Stephen Széchenyi and the Awakening of Hungarian Nationalism* (1968) and continued with Paul Bödy's *Joseph Eötvös and the Modernization of Hungary* (1972), Béla Király's *Ferenc Deak* (1975), and Istvan Deak's recently published *Lawful Revolution: Louis Kossuth and the Hungarians, 1848-1849* (1979).

Decsy focuses on Andrassy's efforts as prime minister to protect the Hungarian state created by the Compromise of 1867 against the revanchist policy of Emperor Franz Joseph, the court party, and Foreign Minister Count Ferdinand Beust, a policy that threatened the stability of the dualistic system by seeking to reverse the results of the battle of Königgrätz. After 1867, foreign policy became domestic policy for Andrassy. The preservation of the Dual Monarchy as a great power was necessary for the survival of the Hungarian state, but only if the monarchy's sphere of interest as a great power was shifted from Germany to the East. As prime minister, Andrassy already pursued the policy that became the leitmotif of his career as foreign minister, opposition to Russia and alliance with Germany. This led to Andrassy's opposition to all of Beust's plans for an Austro-French alliance directed against Prussia before 1870 and to his advocacy of simple neutrality at the famous meeting of the ministerial council of July 18, 1870 held to decide the Dual Monarchy's policy in the imminent war between France and Prussia. That Andrassy's point of view prevailed against the armed neutrality desired by Beust and the immediate mobilization and intervention urged by the court party is a measure of the influence that Andrassy had acquired and that allowed Hungary to veto foreign policy objectives inimical to Hungarian national interests as perceived by the Magyar ruling oligarchy.

Decsy's story is a familiar one, but his multi-archival research in Austrian, Hungarian, and German archives and his use of a large amount of published material (profusely attested to by forty-five pages of footnotes and an exhaustive bibliography) lend depth to the developments he describes and restores to them the complexity that has been

smoothed over in numerous retrospective historical accounts. With regard to the latter, the author comments critically on previous historiographical interpretations of the Andrassy-Beust conflict. On the whole, Decsy's judgments and conclusions are fair-minded and judicious. He shows that Beust was neither unqualifiedly revanchist nor uninterested in the East and that Andrassy was neither unconditionally pro-Prussian nor neutralist in the summer of 1870. The viewpoints of both men were characterized by inconsistencies and contradictions as they sought to influence each other and master a complex and fluid situation. One of the chief values of Decsy's monograph is the link he establishes between domestic structure and foreign policy. Unquestionably, Andrassy was an adroit politician, but his victory over Beust was as much a result of the inherent logic of dualism as of his political ability. Decsy's early chapters provide some interesting insights into Andrassy's transition from a Hungarian freedom fighter against the Habsburg dynasty in 1848-49 to a pillar of the status quo as Hungarian prime minister and Habsburg foreign minister.

Decsy ends his study on an ambiguous note. He clearly has a high opinion of Andrassy as an "aggressive, self-confident and flexible" statesman (p. 115) who ably defended Hungarian national interests. At the same time, the future foreign minister's greatest achievements masked "the tragic premonition of a not too distant future" (p. 116). One senses here some deeper problems of Hungarian and East Central European liberalism and modernization, which, it may be hoped, Decsy will explore further, perhaps in a biography of Andrassy. A modern biography of the statesman in a Western language is badly needed and the book under review shows that he is well equipped for the task.

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LADISLAV LIPSCHER *Verfassung und politische Verwaltung in der Tschechoslowakei, 1918-1939*. (Veröffentlichungen des Collegium Carolinum, number 34.) Munich: R. Oldenbourg Verlag, 1979. Pp. 209. DM 55.

In this specialized monograph appearing in a series published by the Collegium Carolinum in Munich, Ladislav Lipscher studies the constitutional and political development of Czechoslovakia from 1918 to 1939. Lipscher's work consists of six chapters covering the construction of the state organization and varied aspects of the Czechoslovak constitutional and legal system. In addition to studying the formal political structure of Czechoslovakia, Lipscher devotes considerable attention to political parties and to individuals, especially President T. G. Masaryk.

Lipscher commands a firm grasp of his difficult subject, but one gains the impression that he has

not always organized his materials as thoughtfully as he might have. One value of his work lies in his stress on comparisons between Czechoslovakia and other Eastern European states and on the legal continuities between Austria-Hungary and Czechoslovakia; but, at the same time, he overemphasizes some of the developments of the postwar state. Does it make sense to talk about the framing of the Czechoslovak constitution without paying detailed attention to the ideas generated by the exile and home resistance movements and to particular issues like the controversial Pittsburgh agreement, which Lipscher mentions all too briefly in his text? Lipscher also examines extensively Masaryk's handling of the office of the president, while devoting too little attention to Beneš's tenure in that post. Many of the problems Lipscher has faced probably stem from the difficulty of integrating a discussion of the formal structure of the constitution and laws with a study of the political realities that shaped this structure and gave it meaning.

Despite the impressive documentation of this work, Lipscher has also probably tried to incorporate too much stray detail. There are numerous instances in areas with which I am familiar in which the text could have been clarified or improved. Josef Scheiner should not be described as a "professional" military man. Likewise, in note 590, General Syrový is identified as an opponent of Beneš; but in note 437, he is associated with that strange metaphysical animal, the "Castle," of which Beneš was presumably an integral part. In more important issues, one finds, for example, a discussion of the 1936 Law for the Defense of the State without relation to its creation of a frontier zone, surely one of its most noteworthy sections and one with serious constitutional implications. The Agrarians Udržal and Viškovský are identified as belonging to the "Castle Wing" of the Agrarian Party, but this "Castle" designation represents almost a *reductio ad absurdum* of what might be termed German Castle theory (*Burgtheorie*). At one point in 1927, Udržal, who had no love for the legionnaires, maneuvered Masaryk into appointing an inspector general of the army whom Masaryk vehemently opposed. Many of these matters, both large and small, might have been eliminated if Lipscher had shifted his focus to a more chronological and less theoretical account of Czechoslovak political developments. This would have helped him to fit together the details of his narrative more carefully than he has done.

Specialists may nonetheless find Lipscher's study a useful reference work. Its extensive documentation supplements existing historiography with helpful discussions of interwar Czechoslovak politics, political figures, and legal developments.

JONATHAN ZORACH

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JANUSZ TAZBIR. *Piotr Skarga: Szermierz kontrreformacji* [Piotr Skarga: Champion of the Counter Reformation]. Warsaw: Wiedza Powszechna. 1978. Pp. 343. 60 Zł.

The Jesuit Piotr Skarga (1536–1612), court preacher to Sigismund III, was one of the great figures of the Polish Counter Reformation. "The chief troublemaker of the Commonwealth" he was called by the Protestant and anti-Jesuit Catholics of the Zbrzydowski uprising against Sigismund III's regime in 1606. It is arguable that his *Lives of the Saints*, first published in 1579 and frequently reprinted, was the most influential book of Polish baroque Catholicism. In the late eighteenth century Skarga's splendid jeremiads, the *Sejm Sermons* (1597), were elevated to prophecy of the partitions. Liberals and Marxists have concurred in making Skarga the moving spirit of the "Catholic reaction," the advocate of persecution, obscurantism, and absolutism.

In 1962, Janusz Tazbir, one of the most prolific Polish early modernists, published a biography of Skarga that dwelt long and censoriously on the Jesuit's central role in the "Catholic reaction." No startling revelations are offered by this new biography (little, it seems, remains to be discovered in the sources), but the tone has softened. The partisan expression "Catholic reaction" rarely appears; instead, the reader is permitted to see the late sixteenth-century revival of Polish Catholicism as an authentic cultural experience, organically related to the Polish Renaissance. One of Tazbir's most effective points is in establishing close parallels between Skarga's social views and those of his heretical opponents. Drawing upon recent Western treatments of Reformation social policy, Tazbir sympathetically discusses Jesuit efforts to organize urban philanthropy, lessen exploitation of the peasantry, and deal rationally with usury.

The old Skarga is still here: the enemy of the Warsaw Confederation who repeatedly threatened dissenters with mob violence, helped force the Orthodox into the Union of Brest, and inveighed against the szlachta's privileges. But Tazbir's restraint permits a realistic appraisal of the Jesuit's attitudes and tactics. Of petty gentry origin, Skarga understood well the gentry temperament and avoided blundering into the pro-Habsburg camp, a mistake that caused other Polish Jesuits considerable political difficulty. Far from championing absolutism (a political system about whose reality in sixteenth- and seventeenth-century Europe Tazbir is skeptical), Skarga advocated reviving fifteenth-century "mixed monarchy," allotting a diminished but not insignificant role to a chastened, law-abiding gentry. Such medicine for the fevers of "gentry democracy" was prescribed by most political writers in Renaissance Poland, regardless of con-

fession. Tazbir skillfully analyzes Skarga's relations with Chancellor Jan Zamoyski and Sigismund III, showing that the king was hardly the Jesuits' puppet and that Skarga adroitly mediated between the court and Zamoyski's opposition. As for the Jesuits' attack on religious toleration, Tazbir holds that Skarga sought the conversion, not the extermination, of dissenters and schismatics and that he was remarkably effective in winning converts.

Tazbir's new biography is an important addition to the literature, full of suggestive insights into the *mentalité* of Counter Reformation Poland. It is heartening that this re-evaluation of the making of Poland's Catholic tradition was written by a prominent Marxist historian and published in a series meant for the nonprofessional public.

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RICHARD A. WOYTAK. *On the Border of War and Peace: Polish Intelligence and Diplomacy in 1937–1939 and the Origins of the Ultra Secret*. (East European Monographs, number 49.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1979. Pp. 141. \$11.00.

This is a short book with a lengthy title on an intriguing topic. The reader's curiosity is whetted by the presumed correlation between the tortuous diplomacy of Poland on the eve of World War II and the work of its intelligence service. In recent years this service has attracted the interest of historians because of a unique asset at the disposal of the Second Department of the Polish General Staff. Known by the name of "Enigma," it was a machine enabling the Poles to decode German ciphers used in radio communications. "Enigma," handed over to the British in 1939, has been recognized as a major contribution to the war effort and is the subject of a number of books. Its story is also a part of this study, broader in scope, by Richard A. Woytak.

What Woytak terms the border of war and peace is the borderline separating Poland from Germany, which grew alarmingly in length with Hitler's bloodless conquests. The book covers the two last years of peace starting with the Anschluss. As the fall of Austria endangered Czechoslovakia, the Polish assumption was that the Czechs would not fight for the integrity of their state's territory and that Poland, itself faced with a growing threat, should secure its interests in the forthcoming crisis. Following the Munich Conference, which disregarded Polish claims to Teschen, Warsaw presented Prague with an ultimatum demanding the cession of the disputed area. In this manner the solution of the Teschen question was achieved, but it did not end Poland's involvement in what remained of Czechoslovakia after Munich. This book on the politics of

the borderlands brings into focus new Polish concerns with developments across the Carpathian range. Unlike other authors, Woytak emphasizes not so much the significance of Slovakia, but rather of Carpatho-Ukraine, an autonomous state that was becoming a center of Ukrainian nationalism. Polish intelligence was soon engaged in subversive activity to assist the Hungarians in their schemes to annex this territory. Hungary was prevented by the Germans from making this move until the final dismemberment of Czechoslovakia, an event that led to the strategic encirclement of Poland.

Despite its title, Woytak's book turns out to deal more with diplomacy than with intelligence, and the connection between the two is not clearly apparent. In effect there are distinct narratives of each. For all the advantages of possessing "Enigma," it does not appear that Poland's diplomacy was to any unusual degree influenced by the achievements of her secret services. In any case, the Germans in September 1939 modified their encoding equipment and the resulting difficulty for the Polish experts was not overcome during the remaining year of peace.

Woytak makes use of an impressive quantity of primary sources and interviews with surviving intelligence officers. Yet, his book may well serve as a reminder of the pitfalls of avoiding comment and interpretation. Stylistically the flow of narration is frequently broken by unexpected twists and switches from one theme to another, while some points seem blown out of proportion. In tune with his strictly factual approach the book ends with no conclusions—instead there is an extensive bibliography. Despite its flaws of structure and style it remains a thoroughly researched contribution on lesser known aspects of Poland's foreign policy in these crucial years.

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R. G. SKRYNNIKOV. *Boris Godunov*. (Stranitsy istorii nashei Rodiny.) Moscow: Izdatel'stvo "Nauka." 1978. Pp. 192. 70 k.

R. G. Skrynnikov's latest work is the best popular book on Boris Godunov since Platonov's classic. In a number of ways, Skrynnikov's approach resembles that of his predecessor. Like Platonov, he presents a picture of Godunov far more favorable than the one immortalized in Pushkin's play and Mussorgsky's opera. Boris, Platonov and Skrynnikov argue, was an intelligent and humane ruler and an attractive human being. He was in no way responsible for the death of the Tsarevich Dmitrii, in fact the victim of an accident during an epileptic seizure. Moreover, they argue, his government worked hard and with

considerable skill to solve Russia's pressing social, political, and diplomatic problems. The regime collapsed under the battering of forces that no ruler of the early seventeenth century could have fought—massive famine and the resulting explosion of social unrest.

Despite the similarity of some of his ideas to those of Platonov, Skrynnikov speaks with his own distinct voice. Like all of his work, *Boris Godunov* offers the reader the fruits of his masterful study of the sources. The author combines new archival material with a careful and imaginative reading of well-known texts to shed new light on a complex and poorly documented period. His analysis of the sources on Godunov's election as tsar and his unraveling of the stories on the early life of Grishka Otrep'ev, the False Dmitrii, for example, are masterful. On other subjects, such as Godunov's legislation on the peasantry, Skrynnikov has left the study of sources to others but presents their conclusions to the reader clearly and critically.

Skrynnikov's work, moreover, shows admirable sensitivity to the complexities of Russian political and social life. He eschews simple sociological statements, rightly criticizing Platonov for claiming that Godunov's policies consistently favored the provincial nobility at the expense of the traditional aristocracy. Social reality and the tsar's attempts to shape it were both much more complex, Skrynnikov insists.

On the negative side, Skrynnikov's book lacks the literary and interpretive strength of its predecessor. Like the author's earlier works, *Boris Godunov* resembles a late medieval chronicle. Each event or problem confronts the reader in strictly chronological order as a discrete entity. As a consequence, it is difficult to see the author's overarching scheme of interpretation. When Skrynnikov offers a general explanation of events, moreover, he all too often descends to the level of truism. Almost every government action of the period, for example, is presented as a response to incipient popular revolt. Personalities, too, tend to get lost in the march of events. Skrynnikov has many interesting things to say about Godunov's personality and political style, but he makes his comments in a series of digressions from his narrative rather than in a sustained portrait.

On balance, the strengths of *Boris Godunov* outweigh its shortcomings. The high quality of Skrynnikov's treatment of sources and the good sense of many of his judgments make this book a valuable addition to the literature on Muscovite history. The proposed English translation will be most welcome.

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T. S. MAMSIK. *Pobegi kak sotsial'noe iavlenie: Pripisnaia derevnia Zapadnoi Sibiri v 40–90-e gody XVIII v.* [Flights as a Social Phenomenon: The Attached Village of Western Siberia in the 1740s to 1790s]. Novosibirsk: Izdatel'stvo "Nauka," Sibirskoe otdelenie. 1978. Pp. 203. 1 r. 10 k.

This work has the potential of being a good monograph. It is, however, marred by the wholesale inclusion, without attribution, of a portion of a previously published work by the same author, T. S. Mamsik. The passage (a detailed discussion of the *kamenshchiki*—peasants living on the fringes of Russian authority in the Altai Mountains) is from her article in *Iz istorii sem'i i byta Sibirskogo krest'ianstva v XVII-nachale XX v.* (1975). There are some differences in phraseology between the two.

This problem aside, the work under review deserves notice as a well-researched study of an often-mentioned but little-examined phenomenon in Siberian history—peasants and others who left their registered places of residence to escape feudal obligations. The study is limited to the area of villages attached to the state-owned Kolyvano-Voskresenskii works in the southern part of the Ob and Irtysh valleys between 1748 and 1796. After the traditional historiographical introduction, the book is unevenly divided into three chapters. The first, and the longest, is a detailed analysis of 2,619 cases of flight (*pobeg*). The material was gathered by painstaking research in both central and *oblast'* archives, primarily from court records. The escapees examined include the local attached peasants (*pripisnye krest'iane*), nonagricultural personnel of the works (*masterovye*), and new arrivals (*prishlye*)—escapees from outside the area but caught within it. The latter constituted less than a third of the total and their numbers significantly decreased over the period. After an examination of the social status of the escapees, reasons for flight, and place of escape, the author turns to an analysis of the means of subsistence of the fugitives while they were "in flight." The sources allow that information to be substantiated in 20 percent of the cases.

In the last two chapters Mamsik attempts to set these fugitives in their historical context. The second chapter is a review of the empire-wide policy against flight and that policy's application in the studied area. Because of the vast territory involved and of the need for a population to develop the area, policy there could not be a replica of that in European Russia. But Siberian officials still faced problems of control and increasingly shifted enforcement measures to the *obshchiny* of the attached villages. A "circular (collective) guarantee" was used to assure the Kolyvano-Voskresenskii works of needed labor. In the third chapter, Mamsik examines the relationship of the peasantry of the villages

to the escapees and finds that the *obshchiny* actually encouraged flight. Behind this lay several reasons: kinship and personal bonds, the folk ethic of helping "runners," an ethic not unconnected to religious motives, and the development of economic ties between the villagers and the escapees. Here she argues that flight became a "social phenomenon." When escapes so depleted the work force that the "circular guarantee" placed excessive demands on the peasants, group devices such as refusals to work and petitions were used. The peasants' attempts to establish work norms based on a "single soul" support this argument. The administration responded with repression. The author's point is that the individualistic act of protest against the feudal order, flight, was transformed into a "social (class) phenomenon."

The argument has some merit. Certainly the peculiarities of the Siberian situation made for an alteration of national policy, and the presence of kinship and personal ties strengthened the *obshchina* against the bureaucracy. The emphasis on the economic relations between the productively engaged escapees, the majority of those whose means of subsistence could be established, and the village is well taken and suggestive. To make an individualistic act an economically determined part of the class struggle seems to stretch the point, however.

Thus the core of the work contributes to the historiography of some aspects of Siberian history—the illegal movement of the nonprivileged sections of society within and into Siberia, their role in the colonization of the area, and their relationship to the slowly developing bureaucracy, as well as to the settled village *obshchina*. The use of an extensive section of previously published prose is misleading, however. A short summary would have sufficed, and attribution is certainly in order, even when the material is from the same author.

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JOHN SHELTON CURTISS. *Russia's Crimean War*. Durham, N.C.: Duke University Press. 1979. Pp. xi, 597. \$29.75.

John Shelton Curtiss rightly anticipates objections from diplomatic historians to certain aspects of this work. Some problems are almost unavoidable in doing a narrative history of such well-trodden ground as this. Well-known events are recounted and familiar diplomatic exchanges summarized, largely from standard published sources. There are mistakes and dubious interpretations as well, due either to overlooking some of the vast literature or relying uncritically on Russian sources. For example, both the Anglo-French-Austrian alliance of

December 1854 and the peace feelers and moves in the fall of 1855 are explained on the basis of Russian versions of events long since refuted from other sources.

But the book should not be judged mainly on such details. Even its diplomatic history sections, the least successful aspect, present Russia's side of the story clearly and convincingly, on the whole. Curtiss's view that the Western powers were more responsible than Russia for the outbreak and prolongation of the war is certainly defensible, and by drawing on Russian archival materials he sheds some new light on Russian attitudes, especially during the peace moves and negotiations of 1855–56. The discussions of military events are very effective both as narrative and analysis, demonstrating Curtiss's expertise as a military historian and his considerable descriptive powers. The final chapters, though presenting no new ideas on the results of the war, convey a vivid impression of its impact on the Russian people and its leaders.

As for Curtiss's conclusions, they are generally reasonable, but perhaps do not go deep enough. For instance, he can plausibly argue that in 1853 Russia was relatively innocent of aggressive intent and represented no overt threat to Turkey's integrity or the European balance. He can even claim (p. 35) that Britain and France had been more expansionist in 1815–50 than Russia (though one is astonished to learn that Austria and the United States were also acquiring colonial empires at this time). But even if Russia mainly wanted to restore and guarantee her previous relation with Turkey, disturbed by France, and even if Nicholas's deepest motive for seeking influence over Balkan Christians was to repress and control revolution (p. 168), the point remains that the status quo ante, in Russian eyes, meant a strong Russia dominating a weak Turkey and holding the threat of Christian insurrection constantly in its hands. Such a status quo was tolerable only so long as it remained unacknowledged, informal. The very attempt to formalize and guarantee it, however intended, was implicitly aggressive, destroying the necessary fiction of Turkish independence and challenging the principle of joint European jurisdiction over the Near East.

While engaging in counterfactual arguments (itself a useful device), Curtiss also sometimes attaches too much significance to contingent events. He argues, for example, that by better strategy Russia could have won the Battle of Inkerman and, thereby, the war. The battle, yes; but the war? Were Britain and France more likely to make peace after a military disaster or to try other and more revolutionary means to wage war? He also suggests that a Russian victory in the war might have prevented the reforms of Alexander II—again, a too sweeping conclusion. The very fact of the war, not simply its

outcome, demonstrated the failure of Nicholas's system and the need to strengthen and modernize Russia to meet the challenge of a hostile Europe.

But these are points to debate, not charges leveled against the book. It remains commendable as a clear, sympathetic, and, especially on the military aspects, interesting portrayal of the Russian side of the war.

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B. V. TIKHONOV. *Pereseleniia v Rossii vo vtoroi polovine XIX v.: Po materialam perepisi 1897 g. i pasportnoi statistiki* [Migrations in Russia in the Second Half of the Nineteenth Century: Based on the Census of 1897 and Passport Statistics]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 208. 1 r. 60 k.

This book's title refers to migrations, and its author carefully delineates the diverse forms and directions of internal migration in prerevolutionary Russia. Using mainly published sources (especially the 1897 census), B. V. Tikhonov examines the origins, destinations, motives, and terms of settlement of various categories of migrants.

For the most part, migration followed a few well-worn channels: to Moscow and Saint Petersburg, to the southern and southeastern provinces, and to Siberia. Tikhonov presents not only the aggregate totals of each province's in- and out-migration, but also the precise origin and destination of each major stream of migrants. In the first of three major chapters, he identifies 276 pairs of provinces that together accounted for over half of all interprovincial migration. The second chapter offers a similar analysis of labor migration; this discussion is hampered, however, by the unsatisfactory categories used by the census-takers to describe the wage-earning population. The third chapter uses a different body of evidence—short- and long-term internal passports issued to workers—to illuminate the connection between temporary labor migration (*otkhodnichestvo*) and permanent resettlement.

Although the author, especially in chapter 3, shows a keen awareness of the subtleties of migrants' life patterns, he has an unfortunate tendency to oversimplify the causes and results of migration. For him, *otkhodnichestvo* is mainly a prelude to permanent out-migration from the peasant villages. Agricultural colonization reflects a broadening of capitalist relations, while nonagricultural migration is a sign of their deepening. The various forms and directions of migration, in other words, are presented as essentially complementary, but to this reviewer the evidence seems unconvincing.

Tikhonov asserts that poor peasants predomi-

nated among migrants but offers little solid evidence. Several recent studies have suggested an opposite conclusion: that migration may have attracted a middle stratum of more productive and innovative peasants. This is an issue worthy of close, careful consideration, but it will not be resolved by aggregate census statistics or passport records.

Another issue that deserves scrutiny is migration's effects on the provinces of heavy out-migration. *Otkhodnichestvo* in particular tended to leave agriculture in the hands of the old, the very young, and the unfit, and migrants' earnings were often sent home to prop up decrepit and unproductive agricultural households. Did this really strengthen capitalism's hold on the countryside? One might conclude that contradictory processes were at work, and that capitalist relations advanced in some regions at the expense of others. Tikhonov, however, barely mentions this possibility.

Using the same sources, Barbara Anderson, Daniel Brower, and other non-Soviet scholars have asked new questions, testing their hypotheses with advanced statistical methods. One can only wish that there could be a closer exchange of ideas between Soviet and Western colleagues.

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LEOPOLD H. HAIMSON, editor. *The Politics of Rural Russia, 1905-1914*. (Studies of the Russian Institute, Columbia University.) Bloomington: Indiana University Press. 1979. Pp. x, 309. \$19.50.

This admirable collection of well-coordinated essays describes and analyzes in detail the political processes of rural Russia in the last twelve years of the Old Regime. The common concern of these essays is the impact of the nobility on Russian politics and the chasm of political cultures between the nobility and peasantry and between rural and urban elements. Their common thesis holds that the obliviousness of the "Janus-faced," estate-service nobility, entrenched in Stolypin's "June Third" system, to the clamor for change is the key to an understanding of the character of the politics of the period and the dynamics of the revolution that culminated in 1917. The studies hold that the resilience of this system lay in the mutual dependence of the government and a nobility vigorously defending its privileged position against attacks from both above and below. They also maintain that contemporary and later generations of analysts of the period have been overly influenced by the assumptions of the intellectual, urban-oriented leadership of the liberation movement who identified its politics and purposes with those of all Russia. With no response

from the countryside to their programs, they could not conceive that rural politics could affect or explain Russia's political destiny.

Caught up briefly in the constitutional movement under the impact of the regime's poor showing against Japan and disgruntled by its new emphasis on industrialization, the nobles were quickly disenchanted by liberal and leftist demands that they yield some or all of their property to the peasantry and by peasant violence against that property. They swung sharply rightward to dominate the zemstvos and to disavow both the Kadets and the flabby, moderately constitutionalist Octobrists, who were trying to bridge the rural-urban gap and were standing on the dangerous ground of diluted expropriation. They merged their efforts in a nationwide organization, the United Nobility, or regionally in the southwestern Russian Nationalist Party. Working in tandem with the State Council, the upper house in parliament, to ward off bureaucratic threats to their control over local self-government, they were significantly instrumental in the promulgation of Stolypin's electoral law of June 3, 1907. They held that their cause represented the interests of the nation as a whole.

From the above it is apparent that in the long perspective since the end of the fifteenth century, at least, the position of the nobility has evolved along much the same lines as in a developing society. It remained essentially an estate-service element during the parliamentary period. State and estate depended upon each other as they had in the acute (sixteenth-century) and chronic (eighteenth-century) crises. Constantly threatened by bureaucracy and peasantry, the nobility had banded together to protect its interests. The nobles had countered the state when it failed militarily and proved incapable of maintaining internal order.

In this connection, the charge that historians, especially in the postwar period, have concentrated narrowly on the operations of the State Duma is somewhat oversimplified. They had to present a concrete picture of this institution in a new area of objective research: the structure, jurisdiction, and limitations imposed on it by a fretting regime that obviously rested on the bureaucracy and nobility. This is significant for an appraisal of the viability of a representative institution in a society with a six-century statist tradition. The regime had dealt with the oppositional zemstvos and State Dumas in much the same restrictive manner. When significant, the general nature of the pressures on the State Duma have been duly noted—as with the western zemstvo and education bills. The intricacies of the development of these pressures are the domain of the specialists. It is important to understand who challenged the *zuby*, and why, in their declining years. After all, there was a Progressive

Bloc and the emperor's last desperate concession was a "duma ministry."

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JOHANN H. HARTL. *Die Interessenvertretungen der Industriellen in Russland, 1905-1914*. (Wiener Archiv für Geschichte des Slawentums und Osteuropas, number 10.) Vienna: Hermann Böhlau Nachf. 1978. Pp. 135. DM 60.

A notable feature of advanced capitalist development has been the movement of businessmen to form specialized institutions to promote and defend their interests. In Russia, limited attempts in this direction date mainly from the 1880s. But the most visible efforts grew out of a response to the Revolution of 1905 and continued into 1917. The stated goal of Johann H. Hartl's study is to provide an overview of the business association movement in Russia, while the unstated purpose is to throw more light on the chronic weakness of the Russian bourgeoisie.

Beginning with a quick glance at the theory of business organizations as pressure groups, the author briefly examines Russian industrialization and entrepreneurial federations before 1905. Following received opinion, Hartl stresses the state's initiating role in the industrialization process and the inter-related deficiency of private enterprise. Correspondingly, despite some relaxation in the Witte era, the state gave local exchange committees and regional industrial federations limited autonomy and often used them as levers for control and policy coordination. The annual meetings of a given association and the actions of its executive committee had real value for industrialists, however. They provided a channel of communications through which petitions to the government could be forwarded and pressure on decision-makers sometimes brought to bear.

According to Hartl, associations of industrialists continued after 1905 to concentrate on informal lobbying and pressuring within the bureaucracy. There the views of business were routinely heard and almost as routinely ignored. Not that business planned it that way. In feverish 1905, leading industrialists who were suspicious of the Kadets tried to build a political party to speak for "the commercial-industrial class," but the "party of plutocrats" shriveled in the heat of electoral competition. Industrialists exercised minor political influence in the Duma or the State Council between 1906 and 1914, even on questions directly affecting their interests.

In his longest and most original chapter, Hartl constructs a typology of business associations that is

combined with some representative case studies. The focus on structure and internal organization shows how weighted voting systems favoring the wealthiest industrialists were partially counterbalanced by permanent executive committees, quasi-independent statistical sections, and the need to attract less affluent members. The empire-wide Association of Industry and Trade, founded in 1906 to tie together regional and functional federations, and the Association of Mineowners of Southern Russia (1874), which served as the model for a host of followers, receive detailed treatment. Finally, the activities of employers' unions, first formed in 1905 to break strikes and coordinate labor discipline, are illustrated by a study of the Society of Moscow Manufacturers.

This is a modest but useful book, based on printed sources. In its careful attention to organizational structure and typology, it compliments the work of Ruth Roosa on the economic ideology of Russian industrialists. The specialist will find the case studies particularly revealing. And if the weakness of the Russian middle class remains enigmatic, there are some interesting hints for further research on relations between business and government.

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JOHN E. BOWLT. *The Silver Age: Russian Art of the Early Twentieth Century and the "World of Art" Group*. (Oriental Research Partners Studies in Russian Art History.) Newtonville, Mass.: Oriental Research Partners. 1979. Pp. 355. \$16.00.

The Silver Age is mistitled in that it implies a broad survey of cultural and intellectual history for Russia prior to 1914. Instead, this volume simply provides a useful narrative description of the World of Art Society (*Mir iskusstva*), which flourished in the 1898-1906 period and then dispersed into emigration and divergent individual paths inside Russia. John E. Bowlt is a well-known critic, publicist, and cataloguer of Russian art in America.

Bowl't's book contains two background chapters on Russian "realism" and "neo-nationalism" before 1900, five chapters on the World of Art's journal, art exhibits, and esthetic views; and seven mini-biographies of Sergei Diaghilev and other artistic luminaries of the society. It features numerous black-and-white illustrations and is based primarily upon recent Soviet and Western secondary sources.

Unfortunately, the book is marred by overly long descriptive passages and the absence of any explicit argument, method, or analytic framework. If there is a conclusion, it is that the World of Art had "historical and historic importance . . . in both 'calen-

dar' time and 'musical' time" (p. 271). There are frequent long lists of proper names followed by "etc." or "et al." and jargon descriptions of paintings in terms of their "energy," "musicality," or "aesthetic tone." Alexander I is said to have liberated the serfs in the 1860s (p. 29). There is little attention to the broader cultural and intellectual trends of the day (especially theosophy), and European influence is consistently underestimated or ignored. Despite these shortcomings, this book remains a solid description of the World of Art for the English-language reader unable to read N. Sokolova's *Mir iskusstva* (1934) or N. Lapshina's *Mir iskusstva* (1977). It is a useful contribution to Russian art criticism.

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GERT VON PISTOHLKORS. *Ritterschaftliche Reformpolitik zwischen Russifizierung und Revolution: Historische Studien zum Problem der politischen Selbsteinschätzung der deutschen Oberschicht in den Ostseeprovinzen Russlands im Krisenjahr 1905*. (Göttinger Bausteine zur Geschichtswissenschaft, number 48.) Göttingen: Muster-schmidt. 1978. Pp. 273.

The four volumes concerning the Livland *Ritterschaft* (Corporation of the Nobility) and nineteenth-century Baltic agrarian legislation that Alexander von Tobien published between 1899 and 1930 have profoundly influenced Baltic German historiography. Tobien's volumes idealized the actions of the Livland nobility and glossed over certain darker sides of Baltic agrarian development during the nineteenth century. Before 1945 this view of Baltic history, which Tobien documented on the basis of extensive research in the archives of the Livland *Ritterschaft* in Riga, was widely accepted by scholars writing in German.

Among the students of the late Reinhard Wittram who have undertaken to revise Baltic historiography since World War II is Gert von Pistohlkors, the author of *Ritterschaftliche Reformpolitik zwischen Russifizierung und Revolution*. Pistohlkors corrects Tobien's interpretation of the motives and goals of the Livland *Ritterschaft* during two crucial periods of reform: first, the 1840s and 1850s and, then, the revolutionary year 1905. In Tobien's interpretation, the Baltic nobility (especially in Livland) acted to defend the interests of all the inhabitants of the three Baltic provinces by working for reform that assured economic progress and social stability in the countryside. Thus he viewed the reforms of the 1840s and 1850s as a means of encouraging the best elements among the Estonians and Latvians to own or lease land and gradually to assume a more responsible role in local self-government. In the second part of the nineteenth and early in the twentieth

century, according to Tobien, only interference by St. Petersburg prevented the Baltic nobility from introducing further reform that would have effectively protected the local population from the corrupting influence of the *chinovnik* and the Russian revolutionary movement.

Using essentially the same materials as Tobien did earlier, Pistohlkors argues that the main concern of the Baltic nobility was not to promote the welfare of the inhabitants of the three provinces but to defend their own monopoly of economic and political control over the affairs of the Baltic countryside. In the 1840s and 1850s the Baltic nobles only agreed reluctantly to minimal reform in response to widespread social and religious unrest among Estonian and Latvian peasants, pressure from the Russian government, and the prodding of a reform party led by Hamilcar Baron von Fölkersahm. In so doing, Pistohlkors points out, they stubbornly defended their own special rights and privileges and generally ignored the economic, social, and political interests of the overwhelming majority of Estonians and Latvians. By the twentieth century the Baltic Germans had few political allies locally. The last flurry of reform activity of the Livland *Ritterschaft* during the revolutionary year 1905, therefore, had little, if any, chance of success, a point Pistohlkors makes convincingly with a careful analysis of the papers of Marshal of the Nobility Friedrich von Meyendorff and of the proceedings of the special meeting of the Livland Diet held in July of 1905.

Pistohlkors relies largely on German-language materials concerning two periods of reform separated by an intervening period of almost half a century. This intervening period is also important. The policies of the Baltic German leadership and the economic and social development of the Baltic region during the 1870s, 1880s, and 1890s can now, it should be pointed out, be studied in some detail thanks to recently published Soviet Russian, Latvian, and Estonian source materials and scholarly literature. Pistohlkors's monograph remains, however, a significant step toward a thorough revision of Baltic German historiography and an improved understanding of the role played by German nobles in Baltic and Russian history at the beginning of the twentieth century.

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V. N. GINEV. *Agrarnyi vopros i melkoburzhuaznye partii v Rossii v 1917 g.: K istorii bankrotstva neonarodnichestva* [The Agrarian Question and Petty Bourgeois Parties in Russia in 1917: Toward a History of the Bankruptcy of Neopopulism]. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otделение. 1977. Pp. 294. 2 r.

For many years Soviet scholars expressed their contempt for the losers in the Russian Revolution by ignoring them. In the past two decades non-Communist political movements have received a more evenhanded treatment. In the Stalin era Soviet accounts made Bolshevik victory seem too easy and inevitable, because the opposition seemed so weak and shadowy. Today scholars take their measure of the enemy, mentioning names and numbers, thereby enhancing Bolshevik stature by demonstrating the truly formidable odds against a successful Bolshevik seizure of power. Non-Communist political movements are still subsumed under the "bourgeois" label, and their programs must still be dismissed as examples of "false consciousness," or errors in logic, but it is now possible to examine the sources of their popular support and to study their organization and ideology. V. N. Ginev's book on the agrarian question is a good example of this relatively new approach.

Ginev is less concerned with the agrarian question in general than with the agrarian program of populist parties. In particular, he focuses on the interaction between the Bolsheviks and their populist allies, the Left Socialist Revolutionaries, in the development of Bolshevik and Soviet agrarian policy up through January 1918. The subtitle of the book, "Toward a History of the Bankruptcy of Neopopulism," is a more accurate description of its contents.

Although Ginev's book is a remarkably full and objective account of this subject, it contains no new revelations and is based entirely upon published sources and Soviet secondary works. It ignores Western research on the subject, like the books by Oliver Radkey and Maureen Perrie. Two features of the book are noteworthy. First, although the author's conclusions remain well within the canons of Communist orthodoxy, the body of the work provokes some interpretations that are not emphasized at the end. Second, he provides a candid and revealing account of the evolution of Bolshevik agrarian strategy and tactics in 1917. It is a novelty to read in a Soviet account of the First Congress of Peasant Deputies in May 1917 that "the representatives of the peasantry as a whole were not ready to adopt Leninist proposals for the public cultivation of landlord land." It is even more surprising to find a Soviet scholar who is willing to describe how Lenin shifted his tactics in response to that situation, moving away from an emphasis on the preservation of large-scale holdings to support for the redistribution and "leveling" of landholding. "Agitation in Lenin's opinion had to support immediately the concrete peasant demands contained in the theses of the First Congress of Peasant Deputies," and "the program of the peasant poor, which is expressed in these theses, could only be fulfilled by the revolu-

tionary proletariat and by its avant-garde, the Bolsheviks."

Of course, Ginev does a fast shuffle on the question of Lenin's consistency, denying that Bolshevik support for small-scale proprietorship was opportunistic or contrary to the whole spirit of Marxism and omitting, for example, Chernov's comment that "Lenin copies out our resolutions and publishes them in the form of 'decrees.'" One could easily conclude from the body of this work that Lenin owed much of his success to the failure of the populists to carry out their own program and his willingness to pick up where they left off. Lenin later admitted that he copied the agrarian program of the Socialist Revolutionaries "without a single change." But, that is not one of Ginev's conclusions. He cannot emphasize the role of chance and political intuition. In his conclusion he states that Lenin's agrarian program and his alliance with the Left Socialist Revolutionaries were a result of deep insights into the true forces of history and the "correct" solution to the problems of the Russian peasantry and, therefore, provide an example for all future Communist revolutions to follow.

There is much that is valuable in new Soviet scholarship, but, unfortunately, it must still be sandwiched between introductions and conclusions frozen in Marxist catechisms and scriptural quotations from Lenin.

GEORGE JACKSON
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T. H. RIGBY. *Lenin's Government: Sovnarkom, 1917-1922*. (Soviet and East European Studies.) New York: Cambridge University Press. 1979. Pp. xvi, 320. \$34.50.

For two decades T. H. Rigby of the Australian National University has been one of the most prominent political scientists specializing on Soviet affairs. As the title of his last chapter, "Some Historical Reflections," suggests, however, his latest book is primarily a historical treatment. Rigby's meticulous care for chronology and documentation is evident in every chapter. When he resorts to comparison, it is usually diachronic, as in his trenchant comparison of the emergent Soviet Council of People's Commissars (Sovnarkom) to the tsarist Council of Ministers. Nevertheless, the historical treatment rests on a framework of administrative behavior, though some political scientists will wish that Rigby had drawn on a somewhat broader range of administrative theory. The limited cross-cultural comparisons he does undertake—notably between the Sovnarkom and the British cabinet—are well informed and judicious.

Rigby is not primarily concerned with demon-

strating institutional continuity between the Soviet and tsarist periods. Indeed, it would be difficult to do so in a volume focusing on the Sovnarkom, for the revolutionary regime necessarily stressed *changes* at this level. Nevertheless, Rigby demonstrates, by painstaking accumulation of biographical data, that the new masters were not very different in *family* origins (though personal experiences, especially for initial commissars, did differ drastically) from their tsarist predecessors. His findings further substantiate the elitist, unrepresentative composition of the early Soviet leadership suggested by earlier scholars.

Unlike some recent researchers on the initial years of the Soviet regime, Rigby does not hesitate to call it oligarchical. It is *within* the overall oligarchical pattern that he seeks the reasons why the Sovnarkom rather than a *Communist Party* organization occupied the key decision-making position. Since a large majority of Sovnarkom members were also top party leaders (at the start prominence in the Bolshevik Party was the prime qualification, though specialized competence *among party members* became more significant by 1921), one may ask whether the particular institutional locus of decision making was very important. Rigby suggests two interlocking reasons that explain the initial saliency of the Sovnarkom, its rapid recession after 1921, and the persisting significance of the early Sovnarkom experience.

Just as the tsarist system had posited an autocrat, Bolshevik legitimizing principles required ultimate authority to reside in the single-party leadership. Hence the Sovnarkom could never become a responsible cabinet in a parliamentary system and for long periods was relegated to routine tasks. Rigby might have strengthened this aspect of his argument by showing how Stalin, as autocrat, reverted to use of government bodies, such as the Sovnarkom, for long periods between 1939 and 1952. On the other hand, early party doctrine did not specifically stipulate a *party* body as the regular decision center. Consequently, Lenin was able to utilize the Sovnarkom apparatus as his personal staff.

Since, in a sense, Lenin *was* the legitimizing principle of the early Soviet regime, his preference automatically placed the Sovnarkom at the top of the decision-making pyramid as long as he could dominate the system. Conversely, his rapid physical decline entailed the Sovnarkom's recession. Rigby's reading of Lenin's personality leads him to regard this development as avoidable. Although an exacting "schoolmaster" in his approach to associates, Lenin welcomed other strong voices in the Sovnarkom—though in fact they were often absent. Yet Lenin was reluctant to appoint a successor as Sovnarkom chairman strong enough to maintain the influence of the government body. I certainly do

not pretend to know enough about Lenin's complex character to advance an alternative interpretation; but it has often been suggested that, expert in bureaucratic politics as Lenin was, he manipulated the system to maintain his own ultimate authority. If that is truly the case, Lenin's example more than his ambivalent and often contradictory pronouncements may have constituted his persisting formative influence on the Soviet regime.

JOHN A. ARMSTRONG
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VOJTECH MASTNY. *Russia's Road to the Cold War: Diplomacy, Warfare, and the Politics of Communism, 1941-1945*. New York: Columbia University Press. 1979. Pp. xix, 409. \$16.95.

Vojtech Mastny's important study goes far toward filling a major gap in the literature on the origins of the Cold War. Exploiting an impressive array of Soviet and Eastern European sources, as well as recently declassified British and American documents, Mastny carefully traces the evolution of Stalin's foreign policy during World War II. He skillfully illuminates the intricate, constantly shifting relationship among the many factors shaping Stalin's calculations: military exigencies and post-war goals; relations with allies and enemies; the problems and opportunities created by Communist parties in other nations. He offers fresh and sophisticated interpretations of such controversial issues as Stalin's attitudes toward the second front and a separate peace, the diplomatic maneuvering at the various wartime conferences, and the ultimate breakdown of the alliance. Perhaps the most interesting feature of the book is its detailed treatment of Stalin's complex dealing with the various groups competing for power in Eastern Europe and his ambivalent attitudes toward Germany.

According to Mastny, Stalin blended the traditional drives of Russian imperialism with his own special brand of ruthlessness. He was the supreme opportunist, an "accomplished practitioner of the strategy of minimum and maximum aims" (p. 309). At the outset, he could think only in terms of the restoration of Russia's June 1941 frontiers. As the Soviet military position improved, his visions of conquest expanded, but even then he was primarily concerned with winning the war as quickly as possible and avoiding confrontation with his allies. His "craving for security was limitless" (p. 41), however, and when Churchill and Roosevelt appeared to acquiesce in his expansionist aims at Teheran and the Normandy landing assured victory over Germany, he set out with determination to construct his empire. He succeeded, perhaps beyond his ex-

pectations, Mastny speculates, but his short-term success ensured his ultimate failure. At first overestimating Allied compliance, when, after Yalta, the West began to oppose him, he overestimated the depth of the opposition. Panicking, he provoked the confrontation he "neither desired nor thought inevitable" (p. 309).

Mastny's larger conclusions should stir renewed controversy on still timely issues. By failing to make clear to Stalin what they would accept, he argues, Churchill and Roosevelt contributed to the Cold War, a result that might have been averted by an earlier and more clear-cut policy of resistance to Soviet expansion. Perhaps so, but Mastny himself admits the difficulties of such an approach and concedes that the question is academic. He further contends that the "Soviet system" was the "cause of the Cold War" (p. 306), a rather narrow conclusion that ignores the larger historical forces set loose by World War II and does not do justice to the subtlety of the book as a whole. Also open to debate are his assertions that American diplomatic failures were the result of "innocence and inexperience" (p. 310) and that Stalin's commitment to the Cold War was irreversibly set by the end of World War II, the latter an argument not supported by evidence or indeed by sustained discussion. These questions aside, *Russia's Road to the Cold War* is an important and challenging book, far and away the best treatment of a complex and controversial topic.

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NEAR EAST

E. ASHTOR. *A Social and Economic History of the Near East in the Middle Ages*. Berkeley and Los Angeles: University of California Press. 1976. Pp. 384. \$18.50.

Since the publication in 1969 of his huge *Histoire des prix et des salaires dans l'Orient Médiéval* and the republication by Variorum Reprints of articles that appeared originally between 1949 and 1977, E. Ashtor has been recognized as one of the pioneering investigators of the medieval Near Eastern economy. His contributions in the history of prices and wages, based upon a close familiarity with the Arabic chronicles and Geniza documents, have been among the first of the kind. Ashtor's attempt to produce the first survey in English of economic and social conditions in the Near East for a nine-hundred-year period, designed at the same time for the general reader, therefore comes as a new and, it must be said, unsuccessful departure.

Beginning with the seventh-century Muslim conquests and their effects, particularly on agriculture,

Ashtor narrates the rise and decline of Abbasid power, a decline imputed in large measure to the insurrection of the Zindj—a "decisive phase in the history of the Caliphal empire," as the author sees it. Concomitant with the decay of central authority was the development of a feudalism that henceforth was only periodically interrupted. ("Bourgeois" ascendancy during the Fatimid period is mentioned as one example.)

Numerous charts and tables are adduced in support of these and the other leading trends that Ashtor identifies, together with observations that, while likely true, are framed in such a way that their opposites cannot also be excluded. "As in any pre-capitalistic economy there occurred changes in production which increased the accumulation of capital, which means the rich became richer and the poor poorer." But certain other changes in production surely had other results. Indeed, the book is full of instances in which the rich too get poorer. The bravery, moreover, with which the author uses his often very incomplete statistics continues, as in his previous works, to be a matter for concern and surprise.

It might well be that the available sources will not allow any book to be what this one purports to be. Even a thin volume bearing such a title needs something on thought and attitudes, clothing, travel, schooling, marriage and children, buildings, science, recreations, and habits. What actually is offered is a commercial, agricultural, and monetary record of the caliphate and the successor states, introduced by summaries of the political and military context. As for "social" history, there is almost none. Moreover, the general reader to whom Ashtor addresses his book is not likely to be interested in what interests Ashtor. Exceptions to this are the political narratives that comprise by far the most engrossing, readable, and—because already available elsewhere—least needed part of the work.

Ashtor's discoveries and his obvious store of knowledge might more appropriately have been imparted in a series of articles. As for a book on this deserving subject, we must wait further.

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HANNA BATATU. *The Old Social Classes and the Revolutionary Movements of Iraq: A Study of Iraq's Old Landed and Commercial Classes and of Its Communists, Ba'athists, and Free Officers*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1978. Pp. xxiv, 1, 283. \$75.00.

Most observers concerned with the seeming advance of communism in the Middle East used to hold to one or the other of two theses. To some,

communism was promoted by the existing socioeconomic structure, which made communism extremely attractive to the masses. To others, the Communists' potential lay in their tight and effective organization, which gave them great advantage over their opponents, who were numerically larger but greatly factionalized. Hanna Batatu accepts the first thesis. In the opinion of this reviewer, he shows that the correct choice is neither of the above.

In Batatu's analysis, capitalist imperialism's incorporation of Iraq into the world market set loose the socioeconomic forces that have determined Iraq's history in the last century or so. The process began with the creation of the "old Landed and Social Classes" that gained the pre-eminent position under the British mandate and dominated the Hashimite monarchy. The tribal sheik was supreme until the mid-1930s, but the last two decades of the monarchy saw the union of the sheiks and the monarchy, supported by nontribal elements, for the purpose of controlling the masses of Baghdad, which had become the major counterforce. The monarchist-sheik alliance, based on the maintenance of privilege, could not succeed. The Communists, led by middle-class elements, began organizing the masses early in the mandate and, despite repression, survived. The example of the Communists was followed by Ba'thists and Free Officers. The result was the destruction of the monarchy in 1958 and the construction of a new society.

The first third of the book deals topically with the "old classes"—the sheiks, the Ottoman official families, the merchants, and the former Sharifian officers. The second third is devoted to a detailed narrative of the activities of the Communists from the earliest efforts until 1958. The last third provides details of the events of 1958 and the following years until the 1970s. An appendix provides materials relating to contacts between Iraqis and the Bolsheviks in the 1920s. Frequent statistical and non-statistical use is made of copious socioeconomic data, including demographic, economic, and biographical. Much of the socioeconomic data and an even greater quantity of information on events is derived from unpublished records and from interviews with participants or first-hand observers. The unpublished records include British diplomatic papers, police reports (both British and Iraqi), and both Communist and Ba'thist internal documents.

Batatu's utilization of his material is as impressive as its mass. Unlike many who address great questions, he has a decent respect for facts. In exemplary fashion, he gives the evidence in detail and argues each point in turn. At the less abstract level, his generalizations are persuasive and illuminating. But, in this reviewer's judgment, his main theses are counter to his own particulars. Exploitation and repression of the masses are depicted as the main

causes of the monarchy's failure, yet the few cases of violent outbreak for which details are available are clearly related to other matters, and governments have been changed by army coup alone, not insurrection. Batatu overstates the Communists, whose leadership is shown to have been rent by rivalries, their ties to the masses of non-class nature. The party indeed has acted as "traveling companion" of elite groups, such as the *Ahālī* group and Free Officers, rather than the reverse. In the case of the Portsmouth Treaty riots, Batatu slights the activities of the other opposition groups and ignores dissension within the cabinet. The statistical data for the comparative assessment of the monarchy and the revolutionary government is inadequate; others have given quite different evaluations. The statistics on land-holdings in 1958 (Tables 5-1 and 5-3) incorrectly call the holdings private holdings, but in fact they include state lands and land of unsettled title. The only certainty regarding land-holding since 1958 is that official information is confusing.

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AFRICA

GABRIEL WARBURG. *Islam, Nationalism, and Communism in a Traditional Society: The Case of Sudan*. London: Frank Cass; distributed by Biblio Distribution Centre, Totowa, N.J. 1978. Pp. xi, 253. \$22.50.

This book consists of four essays that revolve around the theme of the impact of Islam upon Sudanese society and politics. They are revised and enlarged from previously published papers. All are concerned with the pervasive power of sectarian Islam in the Sudan, which undermined or overwhelmed, depending upon the situation, the Sudanese intelligentsia, represented either by the members of the Graduates' Congress or the Communist Party, who sought unsuccessfully to become independent from the religious sects.

Gabriel Warburg's first essay, "From Ansar to Umma, 1914-1945," describes how the Ansar, under the surviving members of the Mahdi, emerged from defeat in 1898 and constant surveillance by the British intelligence department from that date on, to become a powerful political force after the First World War. This essay is a brilliant analysis of that transformation in which the relations and interaction between the leader of the Ansar, Sayyid Abd al-Rahman, the government, and the leader of the rival Khatmiyya sect, Sayyid Ali al-Mirghani, are intimately woven against the background of Anglo-Egyptian rivalry. Warburg's two main conclusions concern the strength and flexibility of pop-

ular Islam and the inability of the authorities to understand the power of popular Islam that led to their failure to formulate policies to deal with it. Instead, the British expended their efforts to encourage the growth of nonsectarian Sudanese leadership, a tactic that in the end proved futile. Although he recognizes the British failure, the author does not describe what alternative policies the British might have employed; but his analysis of these themes in the preindependence period in the Sudan are accomplished with penetrating power and persuasiveness.

The second essay is concerned with Ismail al-Azhari and the struggle for independence. Warburg sees al-Azhari as a symbol of the failure of the intelligentsia to break loose from the religious grip of the Sayyids and be successful secular nationalists. His conclusion that the Ansar and the Khatmiyya were always behind the scenes in the crucial decisions of the nationalists is as perceptive as it is irrefutable.

The second part of the volume consists of two essays, one on the rise and fall of the Sudanese Communist Party, the other on four aspects of Sudanese Communist ideology and practice. Here the author analyzes the role of the Communists in Sudan politics and their attitude toward Egypt, Arab unity, Islam, and military coups. Unlike the first two essays that are based on extensive documentary sources, many of them secret British intelligence reports, the essays on the Sudanese Communist Party are the work of a historian-detective coming to conclusions, undoubtedly sound, on the basis of public documents. But the information simply is not available to expose the internal workings and personalities of the Sudanese Communist Party.

As a package, these four essays are an important and scintillating analysis of Islam, nationalism, and communism in the Sudan, completed by a final chapter of five documents published by the Sudan Communist Party during the crucial years 1970-71.

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BARBARA E. HARKELL-BONDI *et al.* *Community Leadership and the Transformation of Freetown (1801-1976)*. (Change and Continuity in Africa, Monographs under the Auspices of the Afrika-Studiecentrum, Leiden.) The Hague: Mouton; distributed by Walter de Gruyter, New York. 1978. Pp. xxi, 416. DM 68.

In many African cities, the day-to-day authority that most of the inhabitants perceive is provided not by municipal institutions but by local community leadership. Freetown, Sierra Leone, has always

had a diverse population. Artificially created in the late eighteenth century as a home for black settlers from Britain and North America, it became, after 1808, the center where recaptives liberated from slave ships by the British navy were settled. Immigrants from the surrounding country were attracted—traders, chiefly Muslims, from a distance, laborers from nearby. Kru laborers from the coast of what later became Liberia also came regularly to work. All of these communities evolved self-contained, self-constituted administrations under their own headmen.

The three authors of this collaborative book—a social anthropologist, an economic historian, and a historian concerned with Islam in West Africa—found a common interest in studying their own particular aspect of these community institutions and have fruitfully combined to describe them synoptically. The authors show how each community gave protection and the support of familiar regulations to its members to help them adjust to life in a strange city. Successive colonial governments faced an unwelcome decision. If they recognized the headmen's unauthorized jurisdictions, they implicitly accepted the humiliating existence of "no-go areas" outside the authority of the colony's legal and administrative system. If they prohibited them, the colonial governments would have had to provide alternative forms of local government that they lacked the means to introduce or enforce. They settled for an uneasy compromise, eventually recognizing and making use of headmen but closing their eyes to the illegal jurisdiction exercised by headmen's courts.

At independence the system continued; the headmen inevitably swept into party politics. But many people felt it perpetuated outmoded divisions within a population that was supposed to be striving for national unity. During the military regime of 1967-68 the office of headman was abolished. But they soon reappeared—indeed, as the authors show, in practice the headmen ignored the abolition decree and carried on as usual.

Collaborative writing needs strict editing, and this volume could have been improved by more of it. The early chapters in particular are disjointed. Information from one is repeated in another, and there is no clear unifying chronological sequence. Perhaps as a result of this lack of overall historical perspective, one important element is missing: the community institutions of the early settlers (in their churches) and of the recaptives. Apart from a brief reference to the Yoruba kingship, no mention is made of the numerous recaptive institutions or of the "Seventeen Nations" that joined them. Nor is Abner Cohen's study of the political role of the masonic lodges in Freetown mentioned.

There is also a needless confusion over nomencla-

ture. Recaptive descendants in Sierra Leone have been labeled "Creole" (today often written "Krio") since the mid-nineteenth century—a label used, for instance, by the most articulate member of their community, Africanus Horton, in his *West African Countries and Peoples* (1868). For reasons inadequately justified, the authors call them "Settlers"—a name usually restricted to the early North American immigrants—thus unwarrantably bemusing the unwary reader.

A final section compares the authors' findings with some similar investigations in other West African cities, giving their useful, worthwhile study a wider relevance; this work will interest and inform those studying the same kinds of urban phenomena elsewhere.

CHRISTOPHER FYFE
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KENNETH LUPTON. *Mungo Park, the African Traveler*. New York: Oxford University Press. 1979. Pp. xxii, 272. \$19.95.

Biography is a difficult genre, particularly when the subject is someone whose career has already been detailed. Kenneth Lupton has avoided most of the inherent problems in this excellent, short, detailed biography of one of the first, and probably the most famous, European explorer of West Africa largely because he maintains the focus on Mungo Park's African adventures. The short, informative sections devoted to Park's early life and his private affairs merely prepare the way for the main theme.

This thoroughly researched biography is a work for the educated reader rather than the specialist, although it holds much value even for the advanced student of West Africa. Lupton writes well and does not intrude to a noticeable extent on the narrative. Three appendixes provide much important information that would have been out of place in the main body. Maps are sadly lacking in most such studies; thus, the three good maps at the beginning of the book are a valuable adjunct.

The most lively and interesting part of the biography is the first section since it concentrates on the earliest of Park's adventures. Sponsored by the fledgling African Association, the young Scottish surgeon, accompanied by two African translators, left Pisanía in the Gambia during the dry season of 1795. He hoped to discover answers concerning the fabled River Niger. The events of this journey, duly noted in Park's journal, rival any encountered by later explorers of Africa. Forced to turn back at Silla with many of his questions unanswered, Park had nevertheless seen the river flowing eastward and had recorded the customs of some of the people of the western Sudan. Park returned to England in

1797, received the accolades of the association, and then transcribed his notes into a very popular book. One of Lupton's significant contributions is his detailing of Park's life during the seven-year interim before the government sponsored the ill-fated second expedition. Park, who wanted to continue exploring either in Australia or Africa, was troubled by conflicting desires as well as the slowness of officialdom. He married and fathered four children. Despite this, the taciturn Scot was so eager to return that he left his family behind even though he knew that the late sailing date of January 1805 placed the venture in danger because of the advent of the rains.

There are but a few negative items to report. At times Lupton intrudes to tell us that Park or someone else had made an error in reporting; yet he provides no explanation on how he arrived at this conclusion. The chapter devoted to Park's attitude toward slavery is unnecessarily long for the conclusion reached. The information about Jenne, Timbuktu, and other places visited by Park are reconstructions from other sources since Park's journals of this part of the second expedition were lost. Lupton's re-creations of what might have occurred at each place are interesting but overly long. The various accounts of Park's death at Bussa, although not fully settling the many questions, are both informative and provocative.

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JOHN DUNN, editor. *West African States, Failure and Promise: A Study in Comparative Politics*. (African Studies Series, number 23.) New York: Cambridge University Press. 1978. Pp. 259. Cloth \$27.50, paper \$7.95.

Whatever happened to West Africa? During the 1960s there was a rush by political scientists and historians to tell us why West Africa got its independence before other black African nations. This was followed by a lean decade as political scientists migrated elsewhere and historians retreated to earlier centuries. Now this gap is partially filled by a new volume of essays edited by John Dunn on seven West African nations. To his credit, he did not omit the critical francophone areas (the essays on Senegal and Guinea are sharply honed additions), but one wonders why this ecumenical attitude did not provide for continental representatives, such as Mali or Upper Volta, to balance the coastal states.

Fortunately, Dunn has not imposed a methodological straightjacket on his contributors; they are free to pinpoint idiosyncrasies in their case studies. Richard Rathbone wisely opts against boring us

with yet another review of Ghana's recent history and instead coolly ticks off what went wrong with Britain's former prize colony. Christopher Allen, by contrast, retells postwar political events in Sierra Leone before analyzing the plight of a nation on the verge of exhausting its resources.

The most stimulating essay in the volume is R. W. Johnson's superb update on Guinea, a difficult country to penetrate, much less to understand. He strikes a moderate stance and demonstrates how historical perspective can help inform an arcane subject; the analysis that emerges is possibly the clearest ever written in English on the theatrical politics of Sekou Touré and his proud but bankrupt nation. Donal Cruise O'Brien's graceful and literate essay on the philosopher-king of Senegal, Léopold Senghor, conveys his first-hand impressions after a four-month sojourn to observe recent political developments in a land whose political subtleties are often misunderstood.

Although the essays on Ivory Coast by Campbell and Nigeria by Williams and Turner contribute fresh insights to our knowledge of these two industrial giants, they suffer from being put into constricting analytical molds. Both essays succeed, however, in explaining why these countries are currently prosperous and why they will probably continue to be the economic pillars of West Africa. Christopher Clapham's essay on Liberia retells a familiar story of the Americo-Liberian elite and its propensity for power and maneuver. The participants should be proud of a solid and intelligent collection that demonstrates the vitality of current British scholarship on West Africa (often overshadowed by Africanists in East and Southern Africa); it should be required reading for anyone visiting for the first time the countries mentioned here.

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ASIA AND THE EAST

WILLARD J. PETERSON. *Bitter Gourd: Fang I-chih and the Impetus for Intellectual Change*. New Haven: Yale University Press. 1979. Pp. xiv, 228. \$22.50.

Willard J. Peterson's study of Fang I-chih (1611–71) contains a most impressive translation of a piece of literature (Fang's "Seven Solutions") that has the quality of greatness and is written in murderous Chinese. Beyond that, Peterson writes profoundly on Chinese thought, penetrates a personality of almost Dostoevskian complexity, and sheds much light on a pivotal period in Chinese history.

During the seventeenth century, great socioeconomic change was accompanied by a major in-

tellectual shift, political upheaval, and the penetration of Jesuit learning into elite intellectual circles. In this study, Peterson is interested mainly in what was on Fang's mind as he helped bring about this shift from the long-standing concern with the metaphysics of moral effort (*li-hsüeh*) to "substantial studies" (*shih-hsüeh*), an approach that became increasingly fashionable in the next century, that attacked the "empty talk" of metaphysics, that regarded philology and a bewildering variety of other topics as its domain, and that emphasized the need for factual or textual evidence. While Fang's famous role as a commentator on Jesuit learning has been treated by Peterson in another study, Peterson here barely mentions it, convincingly implying that other problems were more basic for Fang.

Illuminating these problems, Peterson exploits the autobiographical import of "Seven Solutions" to show how Fang defined his life possibilities, ranging from dissipation to scholarship. Most striking is the diffuse sense of frustration with which Fang negated one alternative after another. Far beneath this brilliant son of an elite family was even the thought that settling into the life of a burgher or landlord to work for his family would be a good way to realize China's most famous virtue, filial piety. Driven, like so many of his peers, by a nervous desire to be like the men "of old," Fang could consider three possibilities more seriously: government service in the Confucian sense, character-cultivation in the tradition of *li-hsüeh*, and the esthetic, somewhat un-Confucian style of the *wen jen* (man of culture). He turned from these to life as a Buddhist monk concerned with knowledge as a form of fact-gathering through which one could somehow reveal morality and reach for some totality of understanding.

Peterson can only partly show how this choice was made. Because Fang I-chih, unlike contemporaries such as Huang Tsung-hsi and Liu Tsung-chou, seems never to have been seriously attracted to *li-hsüeh*, Peterson's materials do not greatly help us understand the meaning of this problematic alternative in Fang's day. Neither does Peterson explain how Fang diverged from his many contemporaries, recently discussed in Lu Pao-ch'ien's study of Ch'ing thought, for whom "substantial studies" also meant writing on the restructuring of the central government. Moreover, Peterson only partly analyzes the socioeconomic changes of the day, even ignoring Li Wen-chih's very pertinent and solid hypothesis about the decline of the "privileged landlord." Similarly, in his revealing discussion of the problem of access to the ear of the emperor, he curiously ignores the central Neo-Confucian vision of access through the *tsai-hsiang* (prime office). Also curious is his attempt to identify the heroic ideal of

the knight-errant with the career, rejected by Fang, of that contemptible person who seeks to ingratiate himself with the powerful. Such reservations aside, Peterson's superb study is must reading for anyone interested in the Chinese tradition.

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ELISABETH CROLL. *Feminism and Socialism in China*. Boston: Routledge and Kegan Paul. 1978. Pp. 363. \$20.00.

Scholarly engagement is necessary, yet the engaged historian always risks looking backward to see his or her own reflection—a mirror image that has been especially distorting in pioneer feminist scholarship on Chinese women. Elisabeth Croll, like others, has turned to the history of women in modern China as a seemingly still untarnished example of a feminist movement that succeeded through its association with a socialist revolution. In the Chinese experience she hopes to find clues to the viability of a feminist strategy that subordinates sexual politics to class struggle and that makes liberation dependent upon structural economic transformation. These frankly stated ideological concerns, however, have not resulted here in a narcissistic theoretical analysis but have inspired a readable introductory survey of Chinese women's emancipation movements since the nineteenth century.

Quite simply Croll is the first to try to put the whole story before us. Sinologues will notice that she has relied largely on translated sources, while being forced to acknowledge that the narrative political history she presents needed writing. The work includes a good, if inevitably static, chapter on women's traditional status. Modern feminism is not presented as a creation of the Communist Party but is set in the historical context of earlier reform and nationalist movements. The Kuomintang government of the 1930s and 1940s, with its reformed legal code and middle-class volunteer organizations, is shown clearly as offering a bourgeois alternative to revolutionary feminism. Within the revolutionary movement itself, Croll documents the unresolved tension between CCP campaigns attacking domestic patriarchy and the social conservatism of the otherwise revolutionary peasantry. Still, with the bourgeois and traditional alternatives in view, readers are unlikely to dispute her conclusion that both socialism and feminism benefited from their revolutionary coalition.

After 1949 Croll's mirror clouds up, partly because of an inability to transcend either the analysis or the data base of her sources. The literature of official exhortation and mobilization vibrated with

campaigns—for women's participation in labor, for raising political consciousness, for equal pay for equal work, for cadre development. Here confused and often contradictory shifts in line show at least that the relevance of women's issues to socialist development was never, in this most experimental of times, officially forgotten. The Great Leap Forward brought millions of women into the labor force; its accompanying drive to socialize domestic labor faltered. Cultural revolutionaries suppressed separate women's organizations but pushed for "affirmative action" goals of female political participation. Were such "twists and turns" caused by the backward drag of traditionalist attitudes, not only among the masses but also within the leadership? Or was there a battleground of conflicting socialist-feminist strategies, pitting separatist against assimilationist, economic determinist against political activist, those who saw accommodation to female backwardness as a necessary adaptation to reality against those who branded it a betrayal? The documentary evidence only hints that such conflicts existed; it points to no clear socialist consensus as to their solution.

Therefore Croll's own tentative conclusion, that by the early 1970s feminism and socialism were achieving a "synthesis," seems premature—a piece of instant history fostered by her own trip to China in 1973. To prove such an ideological resolution would require a better comprehension of the Mao era than the murky last years of the "Gang of Four" could possibly afford an observer writing in 1975. To prove that social reality now approaches a Maoist ideological vision would require that we shift perspective from politics to the sociology of women's emancipation in China, an inquiry that since Mao's death is just beginning.

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HOLMES WELCH and ANNA SEIDEL, editors. *Facets of Taoism: Essays in Chinese Religion*. New Haven: Yale University Press. 1979. Pp. 302. \$22.50.

"Religion" is a key word in this book's title because it signifies that the subject treated is religious and not philosophical Taoism. These two thought systems, though historically and intellectually linked, are also sharply different. Philosophical Taoism began in the fourth and third centuries B.C. with Lao Tzu and Chuang Tzu and thereafter enormously influenced Chinese life and thought. Religious Taoism began in the late second century A.D. as a socioreligious movement and thereafter developed into a major religion with temples, priests, a large pantheon, and beliefs stemming not only from philosophical Taoism but also from folk religion, sha-

manism, and Buddhism. It furthermore fostered techniques, notably alchemy, for the achievement of long life or immortality. Until fairly recently, philosophical Taoism received much more attention from Western scholars than did its religious counterpart. The present book is notable, therefore, as the first major scholarly production in English wholly devoted to religious Taoism.

The book's lucid editorial introduction and its nine subsequent studies are outgrowths of a 1972 symposium on religious Taoism held in Tateshina, Japan. Its cosmopolitan spirit is manifested by the fact that several of the studies were originally written in Japanese or French and that its editors and contributors include five Japanese (H. Miyakawa, T. Noguchi, N. Ōfuchi, T. Sakai, and the late Y. Yoshioka), four representatives from France (C. L. Hou, actually a Taiwanese Chinese, M. Kaltenmark, A. Seidel, and R. A. Stein), and three from the United States (R. B. Mather, M. Strickmann, and H. H. Welch). Unfortunately, efforts to gain the participation of scholars from mainland China proved unavailing.

Chronologically, the studies range from analysis of a religious scripture of possibly the second century A.D. to observations about life in a major Peking Taoist monastery during the 1940s. Two studies are bibliographical: one on the complex and voluminous *Taoist Canon*, the other on Japanese studies of Taoism during the past century. The editorial introduction also concludes with a brief list of major French publications on Taoism from 1972 to 1977.

It should be stressed that the book is highly technical. All of its studies represent good or excellent scholarship, but some have such an abundance of detail that it will probably be difficult for non-specialists to see the forest for the trees. Somewhat easier reading in this respect are Hou's paper on Chinese beliefs in baleful stars, Mather's on the fifth-century Taoist theocracy of K'ou Ch'ien-chih, and Strickmann's on the slightly later alchemy of T'ao Hung-ching.

Strickmann's proposal (pp. 165-67), however, to redefine the word "Taoism" by limiting its use to religious Taoism only, while dismissing early philosophical Taoism as a mere "prehistory of Taoism," is an attempt that strikes this reviewer as both narrow-minded and presumptuous. After all, the term *Tao chia*, meaning a Taoist school of thought, occurs in Chinese writings (for example, *Shih chi*, chap. 130) a full two and a half centuries and more before the religious movement (late second century A.D.) with which Strickmann would begin use of the word "Taoism." This reviewer will be astonished if the proposal gains support from any significant number of scholars.

In sum, this book adds notably to scholarly

knowledge about an important but hitherto poorly explored aspect of Chinese civilization. One hopes it will inspire sequels.

DERK BOUDE
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DAVID KOPF. *The Brahmo Samaj and the Shaping of the Modern Indian Mind*. Princeton: Princeton University Press. 1979. Pp. xxiii, 399. \$25.00.

Founded in Calcutta during the second quarter of the nineteenth century, the Brahmo Samaj (Society) consisted of a section of the emerging Western-educated elite who, influenced largely by Unitarianism and philosophic liberalism, reinterpreted their Hindu traditions along monotheistic and rational lines. Accordingly, they rejected idol worship and other rituals of popular Hinduism and, more importantly, promoted far-reaching reform of the Hindu social system, placing particular emphasis on the education and general emancipation of women. Although few scholars will endorse David Kopf's sweeping assertion that the society "played a crucial role in the genesis and development of every major religious, social, and political movement in India from 1820 to 1930" (p. xiii), the Brahmos represented the most radical wing of the modernizing spectrum in nineteenth-century India and had a substantial effect on the intellectual and social life of urban Bengal until the beginning of the century. As such, the Samaj has long merited scholarly attention and Kopf's masterly study, based on a thorough examination of English and Bengali sources, fills a major gap in the intellectual history of modern India.

Kopf gives little attention to the structural or institutional history of the Samaj, which even at its peak never had more than a few thousand members distributed among some two hundred local branches. Instead, and more interestingly, he focuses on the philosophic and theological ideas of leading Brahmos and their role in social reform and the intellectual renaissance in Bengal. He categorizes the Brahmos into three main types, devoting a separate section to each. The first type consists of Brahmo modernizers or "liberals" who were most closely attuned to Western progressive ideas. They were mainly the later generation of Brahmos associated with the secularist and reform-oriented Sadharan Samaj established in 1878 following the second major schism in the movement. This group included two internationally recognized scientists and a distinguished surgeon as well as prominent lawyers, philosophers, journalists, social reformers, and political leaders. Kopf makes a valuable contribution in his perceptive analysis of their ideas and activities. The book's second part deals with those

Brahmos who had greater misgivings about foreign dominance and Western-inspired social reform. Generally labeled "conservatives," this faction, which generally followed the leadership of Devendranath Tagore and formed a separate Adi Samaj in 1866, Kopf more appropriately characterizes as "cultural nationalists"; he emphasizes their importance in defending reformed Hinduism against the Christian missionary assault. Part three, the most original and interesting section of the book, concentrates on attempts to reconcile modernization and nationalism, firstly, in the Brahmo-neo-Hindu evangelism in rural Bengal; secondly, in the activities of Keshub Chandra Sen, the charismatic leader who dominated the Brahmo movement in the 1860s and 1870s; and, thirdly, in the ideas of Rabindranath Tagore, modern India's most famous poet and writer. The theme of Brahmo evangelism has not been previously explored, and Kopf's portrait of Keshub as an Indian prophet of a universal religion of harmony rather than as a near-Christian is quite convincing.

In his passion for his subject Kopf exaggerates the influence of the Samaj, often neglecting the broader influences of Western education. There seems to be an assumption, for example, that the activities of every Brahmo were shaped solely by his Brahmoism. At other times, as in the chapter on Brahmo evangelism, the line between Brahmoism and Vaishnavism seems very thin. On more specific issues, although it is appropriate to designate the reformist Brahmos as "liberals," to characterize them further as "non-nationalists" is to confine one's interpretation of nationalism to anti-imperialism. Many of them clearly had a concept of an Indian nation. Also, Kopf's justification of Keshub's giving of his daughter as a child bride to a feudal prince, while interesting, is not convincing. More analysis of why the movement ceased to be a dynamic influence after the late nineteenth century would also be valuable.

EDWARD C. MOULTON
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ALGERNON RUMBOLD. *Watershed in India, 1914-1922*. London: Athlone Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xii, 344. \$32.25.

Sir Algernon Rumbold was employed in the India Office in London from 1929 until the attainment of Indian independence in 1947. Since his retirement, he has devoted his energies to reflections on the way in which Britain's Indian empire ended. His principal conclusion is that it need not have happened. "Autocracies are not necessarily doomed to die," Rumbold says, "There are still subject peo-

ples . . . there is no principle of inevitability operating against men ready to rule over others who are willing to be bidden" (p. 321). Answering the implicit question of what went wrong in India led Rumbold to the period of his scholarly researches, 1914-22. In these years, shortly before his own arrival at the India Office, Rumbold argues, British officials played their hand so poorly that Indian awe of British omnipotence was irretrievably undermined.

Rumbold has done his research carefully and presents his findings in a forceful, elaborately documented argument. His book thus deserves careful study, even though his conclusions will strike many readers (as they do me) as often deplorable or even bizarre. Mahatma Gandhi, we are assured, for example, could easily have been rendered ineffectual at any time by a display of British firmness. Only the sapping of Britain's willingness to act with confident ruthlessness doomed British rule. In Rumbold's words, "the worst of the wounds which eventually proved fatal to the Indian Empire were self-inflicted between 1916 and 1921" (p. 322). The man Rumbold holds mainly responsible for destroying Britain's will to rule is Sir Edwin Montagu, Secretary of State for India from 1917 to 1922. Rumbold has little good to say about Montagu, either as a policy maker or as a person. "The darting qualities of his mind," he writes, "the belief that he was open to flattery or pressure and his lack of anchorage, introduced an uncertainty into Indian policy which made him suspect. . . . Instead of welcoming advice, his almost feminine instinct was to attribute malice or stupidity to those who counselled caution" (pp. 101-02).

As if having Montagu at the helm of Indian affairs in London were not bad enough, in 1921 an Indian viceroy was appointed who was known publicly as Lord Reading, but who was born with the name Rufus Isaacs. "Isaacs had risen by his talents alone. That would not in itself have been a handicap in ruling the Indian Empire. The fact however that he was of Jewish race, and that he was appointed at a time when the Secretary of State for India was also of Jewish race, roused fears that, without the ballast of tradition, the two between them might embark on ill-thought-out ventures" (p. 242).

Rumbold's book explains and exemplifies the strengths and the limitations of the class of British officials who clung to power in India until the bitter end. Self-assurance, diligence, contempt for "politicians" of the ilk of Gandhi and Montagu, and a certainty that chaos was the only alternative to autocracy were characteristic of this class and are characteristic of *Watershed in India*. Anyone who has trouble understanding how and why the British held onto India long after the compromise of their

capacity to innovate constructively there would be well advised to read this book.

In conclusion, Rumbold notes regretfully that, if Indian independence had been averted, "the middle of the twentieth century might not have seen the massive move of dependent peoples to sovereignty" (p. 322).

FRANCIS G. HUTCHINS

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B. R. TOMLINSON *The Political Economy of the Raj, 1914-1947: The Economics of Decolonization in India.* (Cambridge Commonwealth Series.) London: Macmillan Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xiv, 199. \$37.50.

The object of this short, readable book is to analyze the decolonization of India in terms of the fluctuating economic conditions that brought about changes in the nature and objectives of British rule in India between 1914 and 1947. The author concentrates on imperial policy and argues that the process of decolonization came about through a series of short-term decisions made by British governments concerned with a limited number of specific objectives and constraints. Obviously, B. R. Tomlinson is not one of those historians who see a grand design in British rule in India. Rather, he contends that the British had no long-term strategy for decolonization. In the twentieth century the empire faced a number of crises, each of which left India's imperial role altered. This steady erosion of the imperial role assigned to India by British policy makers was the most important single regulator of the development of constitutional reform.

India's role in the imperial system had three aspects: commercial, strategic, and monetary. This role, however, only became apparent at times of imperial crises: the two world wars, the trade depression of the 1920s, and the Great Depression of the 1930s. Before dealing with the impact of these crises on India's imperial role, Tomlinson sets the stage by analyzing the political economy of the Raj in 1913. Until that year, the impact of a growing international market for Indian produce helped to strengthen rather than weaken traditional agencies such as the indigenous Indian banking system. India's imperial commitment then meant three things: India had to be a market for British exports, so the government of India was not to impose tariff barriers to trade; the Indian Army had to be available for imperial purposes; and the government of India had to ensure the repayment in sterling of interest on government bonds. In the remaining three chapters Tomlinson shows the impact of world events on each aspect of India's imperial commitment in a period when the demands of Indian

nationalism also became a force to be reckoned with.

The author does not claim to have written a definitive study of the economics of decolonization, but this does not deprive his work of value. One of its most useful features is the way it ties changes in the imperial relationship to the institutions that integrated the Indian economy and connected it to the outside world. For example, Tomlinson shows how, in the period between the wars, the impact of a depressed world demand for Indian produce on the internal credit networks was to make the money market in India better integrated than before, with a definite switch of internal investment out of agriculture and trade and into industry. Such changes had an important effect on Indian and imperial politics and government, and these are dealt with in chapters on the relationships between the British and Indian governments, and between the latter and its subjects. In a postscript, Tomlinson returns to his principal theme: the cause of decolonization must be traced to the changes in the structure of the Indian economy and its relationship to the imperial and Indian economies. This theme is well grounded in a solid base of documentary source material and is argued with admirable clarity and economy. I strongly recommend this book to Indian and imperial historians.

PETER HARNETTY

University of British Columbia

GOWHER RIZVI *Linlithgow and India: A Study of British Policy and the Political Impasse in India, 1936-43.* (Royal Historical Society Studies in History Series, number 13.) London: The Society; distributed by Swift Printers, London. 1978. Pp. x, 261. \$20.50.

Of the British viceregalities that occurred during periods of crisis in India, Lord Linlithgow's administration deserves special attention. Gowher Rizvi's aim is both to analyze and to humanize Linlithgow, partly because of Linlithgow's unsympathetic image as a hard-lining imperialist. Rizvi skillfully blends official correspondence from British collections, viceregal and otherwise; and he sheds both fresh and provocative light on Linlithgow and his times.

Linlithgow began in 1936 with the challenge of implementing the Government of India Act of 1935, which provided for democratic Indian ministries in the provinces of "British India" and for a federation combining representatives of those provinces with representatives of the princely, largely autocratic "Indian States." By early 1939 Linlithgow had succeeded with the ministries, but the controversial federation still eluded him. Rizvi's assessment of Linlithgow's inability to secure the

federation is less persuasive than H. V. Hodson's *Great Divide: Britain-India-Pakistan* (1971). (For example, compare Hodson, pp. 58-60, with Rizvi, pp. 74-78.) But from 1939 onward Linlithgow had more pressing concerns. When the Second World War broke out, Linlithgow patriotically declared India at war with the Axis powers. The Indian National Congress's ministries in six provinces resigned in protest at this unilateral decision. Congress made a condition of its further cooperation that Linlithgow and the British government take immediate steps to assure India of postwar independence, the most important step being to convene a constituent assembly. Linlithgow balked. He was fighting a war and had never favored Congress aims. Now, relieved of Congress ministries, he was in no hurry to accommodate Indian nationalism. He was equally quick, as Rizvi demonstrates (pp. 106-23), to co-opt M. A. Jinnah and the Muslim League to the British side—widening the existing gap between the League and the Hindu-majority Congress, underwriting the League's quixotic claim that it represented all of India's 26 percent Muslim population, and encouraging the League's demand that autonomous Muslim provinces be created if necessary to counteract the Hindu majority in the subcontinent. In other words, Linlithgow instituted his own version of that recurring British expedient in India, *divide et impera*.

Readers interested in India's progress toward partition in 1947 will find Rizvi's work thought-provoking. His eagerness to be fair to Linlithgow leads him, however, to overstate parts of his case. Although he deplores a search for scapegoats, he tends to attribute most of the blunders of the period to the Indian National Congress. For example, he repeatedly blames Congress leaders like Gandhi and Nehru for actions that, if hasty or lacking in the studied pragmatism of their Muslim antagonist, Jinnah, were based on the accurate observation that most British politicians, including Linlithgow, were unwilling to underwrite national or democratic goals for India. Congress impatience led them to overreach their resources for a revolution. But on Rizvi's own carefully marshaled evidence, Linlithgow's purposeful cultivation of Jinnah and the League during 1939-42 did at least as much as Congress bungling (or indeed Jinnah's cleverness) to thwart the emergence of a territorially integrated Indian nation. Because of Linlithgow's uniquely privileged position as viceroy, it is hard to accept Rizvi's rather complacent tendency to lay the main responsibility elsewhere. Rizvi himself makes clear that when Linlithgow left India in 1943 he had pushed the Muslim League and the country measurably down the road toward "Pakistan."

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CARL A. TROCKI. *Prince of Pirates: The Temenggongs and the Development of Johor and Singapore, 1784-1885*. Singapore: Singapore University Press, for Institute of Southeast Asian Studies; distributed by Ohio University Press, Athens, Ohio. 1979. Pp. xxi, 251. \$12.00.

Nineteenth-century Johor was unique among the Malay states in several respects, and Carl A. Trocki's well-written study brings out clearly the main reasons why. For one thing, its rulers—known first as temenggongs and later elevated to the status of sultans—were especially enterprising and distinguished characters. The temenggong in the traditional Malay state was a senior official with a variety of administrative and ceremonial duties, but by the end of the eighteenth century the temenggong of Johor had come to specialize in something like piracy, being chief of marauding bands of sea people (*orang laut*) who operated in and around the Straits of Malacca and the South China Sea.

The founding of Singapore by the British in 1819 did not at once disrupt the temenggong's business; indeed, it may have doubled it. His successor, though acting as Singapore's official suppressor of piracy, was himself described unofficially in 1836 as "the chief pirate." But the successful growth of Singapore as an entrepôt, the discovery of gutta-percha in the forests of Johor, and the spread of pepper and gambier planting by Chinese immigrant farmers across Singapore island and on the mainland of Johor in the 1840s combined to open up a brave new world for the temenggongs, enabling them to abandon maritime enterprise for territorial rule and in the end to build a model Malay state.

The Chinese plantation system in Johor became the basis of the wealth and power of its small Malay ruling class. It was a system fortunately unbedeviled by the intra-Chinese group feuds that characterized the turbulent frontier of the tin-mining Malay states to the northwest; and this circumstance, combined with the modernizing ability and political acumen of its rulers, enabled Johor—despite its proximity to the headquarters of British administration in Singapore and its continuing dependence upon Singapore's economy—to retain internal independence long after other Malay states had been brought under British control. The wealthy and astute Abu Bakar (1862-95), most illustrious of the nineteenth-century temenggongs and, indeed, "the single most important Malay political figure of the century" (p. 118), first assumed the grandiose title of Maharajah, became powerful enough to deal directly with the colonial office in London over the heads of the local British administration in Singapore, and was officially recognized as sultan in 1885.

Although Trocki's study owes something to the earlier work of such authors as Nicholas Tarling, Mary Turnbull, and Eunice Thio, it is enlivened by a freshness of approach, and its value as a contribution to the subject is enhanced by his use of the state archives to examine in depth the operation of the *kangchu* system by which Chinese entrepreneurs were licensed not only to open up the river valleys of Johor to commercial plantation but also to raise taxes and maintain law and order. It all provides a striking example of that cooperation among the interests of British colonialism, the Malay ruling class, and Chinese immigrant enterprise, which shaped not only twentieth-century Johor but also modern Malaysia as a whole.

BRIAN HARRISON
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BARBARA WATSON ANDAYA. *Perak, The Abode of Grace: A Study of an Eighteenth-Century Malay State*. (East Asian Historical Monographs.) New York: Oxford University Press. 1979. Pp. xviii, 444. \$34.00.

The Malay state of Perak on the west coast of the Malay Peninsula is a typical riverine sultanate. Barbara Watson Andaya has written the history of this state in the eighteenth century, focusing on the half century from 1746 to 1795. These were the years in which the Dutch East India Company had a treaty arrangement with successive Perak sultans. Under this treaty the rulers of Perak delivered tin exclusively to the company, while the company constructed a fort near the mouth of the Perak River to protect the sultanate against threats from sea raiders, especially Buginese and Minangkabau, and to enforce its tin monopoly. Despite the continuous failure of the Perak rulers or the company to eliminate smuggling, the perpetual grievances and negotiations about the price of tin, and the dismal failure of the company fort to provide protection in moments of crisis, the treaty seems to have been important to the Perak sultans. The reason for this, we are told, is that Perak was a weak state. When the company declined and disappeared at the end of the eighteenth century, the fate of Perak was sealed, for in the nineteenth century (not treated in this book) Perak was ravaged by invasion and internal dissension. Perak Malays in these later years looked back to the eighteenth century as a golden age in their history.

Since the available published information on Malay sultanates in the eighteenth century is rather small, this book expands our knowledge and provides interesting insights into the functioning of a rather average Malay sultanate. The fact that a major collection of source materials, namely the company records, is external to the inner workings of

the sultanate tilts the account in the direction of diplomatic-commercial history. The author's efforts to balance this through the use of Malay records, especially the contemporary *Misa Melayu*, is not totally successful. This chronicle is without a doubt a fine example of classical Malay writing, but precisely for that reason it should have been subjected to more rigorous questioning than it has here received. Instead of repeated eulogies of the splendor and glory of the court of the Perak sultans, the reader might wish for more consideration of why this well-to-do state was so incapable of organizing its human and material resources toward self-survival.

ROBERT VAN NIEL
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UNITED STATES

JOHN HIGHAM and PAUL K. CONKIN, editors. *New Directions in American Intellectual History*. Baltimore: Johns Hopkins University Press. 1979. Pp. xix, 245. \$15.00.

This anthology—dedicated to Merle Curti—is close to an “official” directive on the state of American intellectual history studies today. Its official quality is strengthened by the presence of John Higham—co-organizer of a 1977 conference where the essays here were first presented as papers and author of a characteristically graceful introduction to the volume. Ever since 1951 and his famous essay on “The Rise of American Intellectual History” (*AHR*, 56 [1950–51]: 453–71), Higham not only has been explaining the field and its past to colleagues but also has been prophetic of its future.

If this volume be taken as prophetic, in the decade ahead American intellectual historians will (1) increasingly use *social science perspectives* in their studies (see especially the essays here of Laurence Veysey, Gordon Wood, and Murray Murphey), (2) focus on ideas of *particular subcommunities* rather than aim for a general “American Mind” (see Thomas Bender’s magnificent essay on intellectual professions and the city, and, for a cogent dissenting opinion, Rush Welter’s “On Studying the National Mind”), (3) be more sensitive to the *media* through which historical ideas are communicated and not uncritically take the printed word for the whole of cognitive reality (see the essays of David Hall and Neil Harris), and (4) continue being *self-conscious*, and occasionally anxious and defensive, about their field (see the critical essay by Thomas Haskell and the piercing “Afterword” of Paul K. Conkin).

The first “new direction” is the most striking. In-

stead of humanistic terms like "mind" and "myth" and "image"—favorites of past intellectual historians—we find here a language grounded in the social sciences—"social aggregates" (Veysey), "modes of communication" (David Hollinger), "disciplinary matrix" (Haskell), "division of intellectual labor" (Murphey), "intellectual hegemony" (Bender), and, of course, "paradigm" (used by almost everyone).

Higham writes that Clifford Geertz was "virtually the patron saint of the conference" (p. xvi). That is not quite true. If Geertz and Thomas Kuhn are noted by an equal number of contributors (six each of the fifteen total), Geertz is mostly relegated to footnotes, while Kuhn's sense of paradigm and paradigm communities exercises a powerful hold on the participants.

Missing, however, is Kuhn on *change* or, for that matter, much detailed attention anywhere in the volume to historical ideas in the process of transformation. Haskell comments insightfully on the statistics of paradigm functioning but confesses that the dynamics of change in Kuhn are a "mystery" (p. 144). I believe Haskell is wrong, but his comment is indicative of a field that has not effectively handled change or strain in historical ideas.

Missing also are newer perspectives from women's history and from studies of minority cultures. A Nancy Cott or a Lawrence Levine have elsewhere gleaned ideas from nonpublic or non-written sources not ordinarily used by intellectual historians. Though there is much discussion of such sources here, there is little actual representation (save for Harris's essay on the half-tone engraving process).

Finally, the volume—perhaps the conference too—might have profited from an *outside* perspective on the field. Only Sacvan Bercovitch of those represented here is not an academic historian, and he is not far removed. Commentary from a Clifford Geertz, a Thomas Kuhn, a Peter Berger, or perhaps a Kai Erikson could have offered a more detached "new direction" for the field not found in the professional historians here.

GENE WISE
University of Maryland

CAROL HYMOWITZ and MICHAEL WEISSMAN. *A History of Women in America*. New York: Bantam Books. 1978. Pp. xii, 400. Paper \$2.95.

This general history of women in the United States will appeal to students because of its readability and to teachers because of its scope and accuracy. Although they are not professional historians, the authors accomplish a difficult task. Drawing upon their reading of specialized historical studies, they

carefully blend multiple themes into a lively and informed history of American women that spans from colonial times to the present. In addition, this reasonably priced text is extremely well written and abundantly illustrated.

Unlike many surveys of women's history, this work gives significant space to the struggles of working-class and black women, as well as to the well-known battles of leading feminists and professional women. The brutal sexual abuse of the female slave is realistically presented and its connection to the idolatrous legend of pure southern womanhood is skillfully shown. The plight of female wage earners, from the early mill-girls to immigrant mothers and daughters, is also discussed, including their attempts at labor organization. The authors seek to highlight the history of the masses of women. Yet they also stress the lives of the individual feminist "greats" and the social movements that they sparked.

The post-1920 discussion of women's history emphasizes the economic and psychological changes that affected women's status. It concludes with an excellent summation of the New Feminism—its origin, ideology, and challenges.

This book should prove to be a popular text for beginning students in women's history. It is especially well suited for study in high school classes. Its breadth of coverage and variety of historical analysis also make it an appropriate choice for undergraduate survey courses.

DEE GARRISON
Rutgers University

DEWEY W. GRANTHAM. *The Regional Imagination: The South and Recent American History*. Nashville, Tenn.: Vanderbilt University Press. 1979. Pp. xiv, 269. \$11.95.

Over the past two decades Dewey Grantham has built for himself a solid reputation as a southern historian. Two books, three edited works, and a large number of perceptive essays and articles on assorted topics constitute the basis for his high standing in the profession. A survey of this well-known historian's work reveals four major themes: the continuity of southern distinctiveness; the convergence in the thinking and behavior of southerners and non-southerners in modern times; the functions of sectionalism in national politics; and the interplay of southernism, racism, and the national behavior in regard to the black segment of American society.

These themes run through this volume's fourteen chapters, which are devoted to specific subjects such as sectional politics, the Bourbons, southern violence, the Black Patch War, Hoke Smith, Ralph Bunche, and Jimmy Carter. All but one of these

chapters have been published before, either as chapters in books, introductions to edited works, or articles in such respected journals as the *Journal of Southern History*, the *Journal of American History*, and the *South Atlantic Quarterly*. Thus, except for the one heretofore unpublished essay on the Little Rock school crisis, all of the material in this volume is easily accessible elsewhere. This accessibility raises the legitimate question as to whether this volume is needed at all. A more important reservation about the advisability of the publication of this volume is related to the original dates of many of the essays. Since some of the articles are nearly twenty years old, many of the interpretations are dated. In fact, Grantham recognizes this and in his brief introductions to the essays he includes the authors and the titles of recent publications so that the reader can update the subject under discussion. One example will suffice. In the introduction to his article on the southern Bourbons, Grantham lists three recent scholarly monographs on the subject and writes: "Readers may be struck by the extent of the fresh scholarly writings on the period dominated by the southern Bourbons." He concludes: "One scarcely needs to add that if the essay of 1961 were being written in 1978, some additional questions would have to be raised and some new interpretations considered" (p. 24). Since this statement is essentially applicable to all his essays, one raises again the reasonable doubt about this volume's value.

Dewey Grantham has been doing original research on the southern Progressives for nearly two decades. Those of us who know him as a scholar have been waiting for many years for the publication of what will surely be a major contribution to southern and national politics. Grantham's attention to several edited works and the volume under review surely has played a role in the delay of his completion of a very important book based upon original research. I for one wish this able scholar would not allow diversions such as this volume to keep him from giving us the benefits of his considerable research and writing talents.

MONROE BILLINGTON
New Mexico State University

JEROME O. STEFFEN, editor. *The American West: New Perspectives, New Dimensions*. Norman: University of Oklahoma Press. 1979. Pp. ix, 238. \$14.95.

In his introduction to this collection, editor Jerome O. Steffen acknowledges that writing in the history of the American West has yet to reach its full potential. Steffen offers here eight articles (one of them his own) as pointers toward realizing that potential. The pieces stem, he writes, from "different but

equally valid categories of inquiry into American western development" (p. 5). Six of the contributors are historians; one is a geographer and one a psychologist.

John Opie leads off with a helpful review of the literature in the field of environmental studies, but his advocacy of ecology as an "alternate interpretative context for frontier history" (p. 25) is less than persuasive and if adopted would surely lead to excessive presentism. Geographer John Hudson argues convincingly for "refocusing the study of frontier populations to include their broader geographic and demographic contexts" (pp. 57-58), while psychologist Roger Barker advances some tentative assumptions about the nature of frontier environments and suggests their consequences for pioneer behavior. Barker's theoretical framework seems overly elaborate for the rather obvious conclusions it yields.

In a provocative piece comparing "insular" and "cosmopolitan" frontiers, Steffen seeks to "generate a larger approach to western history" (p. 94) and to help re-establish "a dialogue between western historians and the rest of the historical profession" (p. 116). His argument must be read to be understood. To understand it, in this reviewer's opinion, is largely to reject it. Reginald Horsman contributes a most useful historiographic essay in which he surveys contours of controversy over the interpretation of white-Indian relations and points out areas of research that need attention. Richard Etulain examines the fusion of western history and fiction through the works of Vardis Fisher, A. B. Guthrie, and Wallace Stegner. His treatment of Stegner is particularly thorough.

Ronald L. F. Davis contends that the history of urbanization in the West lacks an analytical framework. In response, he proposes that the western city be viewed as "an attribute dependent upon the existing and preexisting modes of production in society [thereby providing] a basis for seeing the diversity and shared character of western cities" (p. 192).

In the last essay Gene Gressley reviews the largely abortive attempts of the West that lies between the Rockies and the Sierras to achieve regional coherence. As population flows into the area and its natural resources continue to dwindle, Gressley sees a chance that the inter-mountain West may shed its localism and join in a regional compact strong enough to offset the centrifugal force of the federal government.

Although the eight essays are diverse in content, method, perspective, and style, one distressing feature marks most of them: the quality of writing is poor. One need not demand the grace of Parkman, but is it not reasonable to expect straightforward, muscular prose that clarifies rather than obscures

the writer's meaning? With two, possibly three, exceptions, the contributors to this collection fail to meet such a standard. To make matters worse the book is pockmarked with typographical errors—more than 60 in only 238 pages. In thirty years of academic life no sorrier piece of book making has come across this reviewer's desk.

EDWIN R. BINGHAM
University of Oregon

JAMES W. CEASER. *Presidential Selection: Theory and Development*. Princeton: Princeton University Press. 1979. Pp. xiv, 371. Cloth \$20.00, paper \$4.95.

Americans have never much liked their political parties, yet few elements of the American political system are as vital as our parties. We are never so mindful of this reality as in presidential election years, for the whole question of presidential selection is inexorably tied to the quality of competition both within and between the major national parties.

James W. Ceaser has transformed a Harvard doctoral thesis in political science into an ambitious and relatively comprehensive historical analysis of the way our political parties have shaped presidential selections. The author takes as his focal problem the failure of the contemporary presidential election system to provide a moderating influence on the pursuit and exercise of power. He candidly espouses a selection system that rewards the politics of moderation, the politics of coalition-building, and especially rewards those who willingly work within the mainstream of party politics. In short, this is a book that contains frequent praise for the party regulars in the Martin Van Buren mode and just as frequent criticism for the pro-participatory activism of recent reformers.

The book is both an exercise in American political thought and a rigorous institutional analysis. Lengthy chapters are devoted to the intent of the Founders, the theories and practices of the Jeffersonian period, the vital role played by Martin Van Buren in nurturing permanent party competition in America, the political thought of Woodrow Wilson, and the evolution of the contemporary party and presidential selection systems. The author's chief contributions come in his helpful examination of evolving theory and practice in the nineteenth century. Ceaser builds upon the work of Richard Hofstadter and Robert Remini to provide a vivid portrait of Van Buren's critical contributions to the art of politics in America.

This study also poses a variety of criteria by which we might evaluate the way we recruit and elect our chief executive. Some readers will doubtless quarrel with certain of the author's values,

which often enter into his evaluative standards, but few will come away from a reading of the book without a richer understanding of many of the dilemmas and paradoxes of the selection process.

Perhaps the most troubling aspect of this book is its topical last chapter, entitled "Modern Party Reform." Here the author becomes an advocate. Among changes he calls for are reductions in or elimination of primaries, lessening or elimination of public financing of presidential elections, and a lessening of "popular" leadership. Political scientist Ceaser apparently yearns for a pre-Progressive-era party system and hopes for an unlikely decided rollback from the democratic impulses of the past decade or two. He may be right or he may be off base—this is for the reader to judge—but the tone is strident at times and not a little jolting after the earlier analysis of historical evolution and political theory.

On balance, then, we have in *Presidential Selection* a readable and informative analysis of a vital American process. It can be read with profit and insight and will surely stir debate and reappraisal.

THOMAS E. CRONIN
University of Delaware and
Colorado College

ABRAHAM S. EISENSTADT *et al.*, editors. *Before Watergate: Problems of Corruption in American Society*. Brooklyn: Brooklyn College Press; distributed by Columbia University Press, New York. 1978. Pp. xiv, 231. \$15.00.

Not too surprisingly, the revelations of Watergate have stimulated a new awareness of corruption in public affairs and of its significance. Perhaps the best indication of this in academe is the conference held at Brooklyn College in May, 1977, with a theme that became the title of this interesting and wide-ranging work. Among the participants was Arthur Schlesinger, jr., whose brief remarks, forming the introduction, begin appropriately enough with a quotation from the Book of Genesis: "And God looked upon the earth, and, behold, it was corrupt." The scholar's task, as Schlesinger observes, is that of seeking to understand the "deeper social mystery" that lies behind corruption (p. 1). He turns to Edward Gibbon, Karl Marx, Robert K. Merton, and others for theories and insights on the subject. Fourteen contributors to the book examine a variety of questions in both the European and American past, although predominantly they concern the United States. All but one of the essays are essentially political in emphasis; four of the fourteen are biographical. Generally the book is loaded with ideas, and a short review cannot possibly do justice to it.

How does one define "corruption"? This is a problem that keeps arising from chapter to chapter and is never settled. Morton Keller in his "historian's-eye view" of American public affairs seems to assume a rather clear distinction between the numerous acts of greed or venality and the more dangerous attempts, occasionally, to usurp power (pp. 8, 16). Edward Pessen, writing on the Jacksonian era, decides that officeholders were frequently guilty of both "venality" and "abuse of power" and that the two were interrelated (pp. 80, 94). Arnold J. Heidenheimer, a political scientist, insists that charges of corruption should be confined to those who have used their positions of public trust for either "direct or indirect material enrichment," while more serious abuses or usurpations should be considered separately (p. 25). In this reviewer's opinion, such a distinction is often difficult, if not impossible, as shown notably in the Teapot Dome scandal and, to an extent, in Watergate itself.

Another theme or recurrent idea is the comparison of corruption in particular instances with other evils whose effects socially may be more damaging. Linda Levy Peck, writing on "The British Case" in early modern history, is one of a half-dozen contributors who cite the influential works of Samuel P. Huntington, a social scientist who treats corruption as a natural ingredient during national growth and modernization. Peck emphasizes, however, that while corruption has its useful purposes and may contribute to political stability, it also can be a "dissolvent," as it was in English history during the 1640s and in British colonial relations in 1776 (p. 47). Four biographical essays strike this reviewer as being among the best. They are: "Albert Gallatin and the Problem of Corruption in the Federalist Era" by Edwin G. Burrows; "Aaron Burr as a Symbol of Corruption in the New Republic" by Mary-Jo Kline; "Carl Schurz, the South and the Politics of Virtue" by Hans Trefousse; and "The Case of Nelson W. Aldrich and the Sugar Trust" by Jerome L. Sternstein. Trefousse describes how Schurz in the 1870s virtually abandoned the cause of newly freed black people and turned his energies instead to fighting corruption through civil service reform—a lesser cause. Good essays by Ari Hoogenboom and Robert Muccigrosso also deal with the Gilded Age. The latter, writing on "Corruption and the Alienation of the Intellectuals," traces forms of cultural protest against "pervasive, corrosive materialism"; one result, of course, was expatriation (pp. 166, 175).

The book contains a few disappointments, one of which is the lack of an index. But, as the editors say in their foreword, the hope is to encourage further research rather than to complete the inquiry here. Heidenheimer is the contributor who seems most concerned at the dearth of scholarly studies in the

United States relating to corruption, and he offers a number of interesting suggestions. A. S. Eisenstadt in a concluding essay raises some questions that ought to be pursued. All in all, this book should serve as a major stimulus to further thinking and research.

J. LEONARD BATES
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ALLAN J. LICHTMAN and JOAN R. CHALLINOR, editors. *Kin and Communities: Families in America*. (Smithsonian International Symposia Series.) Washington: Smithsonian Institution Press. 1979. Pp. 335. Cloth \$17.50, paper \$7.95.

Important topics are the product of important questions. Historical study of social mobility produced some of the most innovative research of the 1960s, partly because it sought to answer a significant question, whether or not America was an open society in which persons could "make it" through ability and hard work. Once the answer to that question came in (a qualified yes), the topic receded in salience, partly because no similarly compelling question emerged to re-focus the research effort.

Family studies may suffer the same fate. They started off in the early 1970s with a significant question—whether or not modernization shattered the cohesion of the traditional extended family and led to the isolated nuclear family. Once the answer came in (the traditional family was less extended and less cohesive than believed; the modern family less isolated), family research has been left without a central organizing question. The result of this void may be the emergence of a lot of research details, but no picture, if the present volume is a harbinger.

In one sense, *Kin and Communities* represents the official arrival of family studies. It is the published outcome of a national symposium held in 1977 at the Smithsonian Institution, sponsored by a raft of corporations (ranging, alphabetically, from American Security and Trust to Weyerhaeuser) and addressed by such personages as Hubert Humphrey, Alex Haley, Margaret Mead, and Rosalynn Carter.

After such a buildup the book disappoints. The problem is not the fault of the contributors. The articles, taken individually, are good, and a few merit special attention. It is simply that the range of topics is so broad, and their focus so diffuse, that the whole is less clear than the parts.

There are ten articles in the volume and seven touch only tangentially on the family. These include Devra Kleinman's study of polygamy among lower mammals, Dale Stewart's sketch of the early trans-Siberian migration that populated the New

World, David Musto's tracing over several generations of the Adams family's high achievement values, David Schneider's unraveling of the "symbolic connections" between kinship and community in American culture, Seena Kolil's study of women pioneers in Saskatchewan, Rosenkrantz and Vinovskis's study of the asylum in antebellum Massachusetts, and Francis Hsu's comparison of individualism and family authority in ancient myths of China and the West. One other essay, Jacqueline Jackson's study of how black families treat their aged members, is more to the point but is fairly unremarkable in its conclusion (that they are treated similarly in both black and white families).

Two essays merit special attention. Glen Elder and Richard Rockwell's "The Depression Experience in Men's Lives" expands upon Elder's path-breaking study of the long-term effects of the Depression on the psychological development and family life of over one hundred adolescents. The present essay demonstrates that the effects were mitigated for those who experienced that event as children rather than as adolescents. The authors conclude that a critical historical event can have quite different effects on people depending on the life-cycle timing of its occurrence. John Modell, provocative as always, throws some cold water on our nostalgic admiration for the cohesion and cooperation of family members in times past. Modell details the enormous disruption of disease, death, and economic distress on families in the nineteenth century and infers that, given the extensive evidence on the darker side of cohesion and cooperation—family tension, working women and children, delayed marriage, sexual frustrations—much that we admire was externally mandated rather than internally motivated.

The second part of the volume contains the results of a series of workshops and colloquia on the value and use of photographs, interviews, and "family documents" in inspiring, researching, and writing family history. There is an extended introduction by the editor that helps impose order on the topical chaos.

LAURENCE GLASCO
University of Pittsburgh

EDMUND JEFFERSON DANZIGER, JR. *The Chippewas of Lake Superior*. (Civilization of the American Indian Series.) Norman: University of Oklahoma Press. 1978. Pp. xiv, 263. \$14.95.

The attention focused by the media on the Indians in recent years has created a climate favorable to intensified research on North American tribes. The resulting spate of histories of individual tribes evinces an attempt to foster a better understanding

of the events and forces that shaped contemporary Indian cultures. In contrast to the histories of only a few decades ago, these new studies draw upon anthropological and ethnological findings and offer more sensitive accounts of Indian cultures. Edmund Jefferson Danziger's study is one of the better examples of this trend.

Dividing the Chippewas into five major groups, Danziger limits the scope of his study to one group, the Lake Superior Chippewas, who live both on reservations and in off-reservation communities south and west of Lake Superior. An ethnohistory of the Lake Superior Chippewas from the first French contact at Sault Ste. Marie in 1641 to the present day, Danziger's work advances no new interpretations. It brings together much of the major research on the Chippewas and offers a solid account of the centuries of Chippewa-white interaction, especially the dealings of the United States government with the Lake Superior Chippewas. Of greatest value are the last chapters, which present Danziger's own synthesis of recent Chippewa history drawn largely from personal interviews with Chippewas.

In covering so long a period, Danziger paints his picture with broad strokes that too often treat cavalierly certain aspects of Chippewa history. For example, the Sioux did not recede in the face of Chippewa incursions into the upper Mississippi Valley as accommodatingly as he implies. But Danziger passes rapidly over inter- and intratribal contact to focus almost exclusively on European and American policy towards the Chippewas. His emphasis is partly dictated by the structure, which follows the process of Chippewa acculturation through the exposition of political and economic events. Much of the evidence of acculturation that Danziger advances, however, is not convincing. While he admits that it is difficult to determine "the rate of acculturation of each Lake Superior band" (p. 107), Danziger does seem to equate change with acculturation and cultural disintegration. As do all people, the Chippewas changed over time, but they did not therefore believe themselves to be any less Chippewa as a result. In fact, the Lake Superior Chippewas continued to cling to old ways far longer than was environmentally viable. Their intransigence led to poverty and to a "white paternalism" that Danziger finds "not unreasonable" (p. 113). Yet one could argue that this paternalism contributed to their continued dependence upon the government and to even greater poverty.

Danziger employs statistics to profile Chippewa economic and social conditions but without sufficiently analyzing their significance. Major concerns, such as Chippewa perception of government policy and the changes and constants of Chippewa culture, do not receive adequate attention. Although it

is possible to quibble with certain statements, in the final analysis Danziger has written a useful book for the study of Chippewa-white relations, especially during the twentieth century.

ROBERT E. BIEDER
University of Illinois,
Chicago Circle

RALPH J. COFFMAN, *Solomon Stoddard*. (Twayne's United States Authors Series.) Boston: Twayne. 1978. Pp. 224. \$9.95.

Solomon Stoddard, pastor of the Congregational church at Northampton, Massachusetts, from 1661 to 1729, was known in the nineteenth century mainly as the originator of the custom of admitting the unregenerate to full communion and as the grandfather of Jonathan Edwards, his successor at Northampton, who reversed his practice. In 1941 Perry Miller wrote a ground-breaking article on Stoddard. More recently, manuscripts of the poet Edward Taylor, pastor in neighboring Westfield, have been discovered that throw light on Stoddard and the communion controversy. Articles by Brooks Holifield, Paul Lucas, James Walsh, and several others have sought to reinterpret Stoddard in light of our expanding knowledge about Puritan theology and social history. The time is therefore ripe for a comprehensive treatment of Stoddard that will sum up these new developments and provide a base line for further scholarship.

Ralph J. Coffman's first chapter brings together practically everything that is known about Stoddard's family background, early years, and education; the second provides important data on Northampton's population and its social and political arrangements. Stoddard's career is seen as having had three successive phases. Each receives an appropriately entitled chapter: Stoddard the "Sacramentalist" tried to cure New England's moral and spiritual ills first by admitting the baptized but unconverted to the Lord's Supper as a "converting ordinance" and then, as "Pope of the Instituted Church," by advocating an essentially Presbyterian polity; finally, "The Evangelical" abandoned both and became exemplar and promoter of an aggressive evangelistic style of preaching that anticipated the Great Awakening.

Apparently aware of the pioneering nature of his biography, Coffman has tried to make it as serviceable as possible. A chronology of major events precedes the first chapter, and each of Stoddard's works (including unpublished manuscripts) is briefly described in its appropriate context. The appendixes contain early verse of Stoddard, a list of his library in 1664, his family tree, charts of "the Northampton Oligarchy," a biographical glossary,

a list of Stoddard's writings, and a select annotated bibliography. There is also an index of names (topics appear only under "Stoddard"). Coffman also cites many sources, such as archives, genealogical registers, manuscript collections, demographic studies, and works by Stoddard's contemporaries.

Unfortunately for Coffman's purpose, his book is poorly written, and typographical errors are legion. There are also gross and multiplied inaccuracies in proper names, titles of works, quotations, references, and bibliographical data and annotations. Interpretive statements contradict the adjoining quotations on which they are presumably based. Scores of examples could be cited.

At the level of interpretation, the author is simply not in command of his material. For example, his effort to demonstrate a later shift in Stoddard from intellectualism to voluntarism is sabotaged by Coffman's confusion of intellectualism with rationalism. Again, there is merit in Lucas's thesis that there were successive emphases in Stoddard's writings and that he became preoccupied with evangelism in his later years, but some aspects of it are debatable. Coffman simply takes the idea and runs with it, making the stages even more mutually exclusive by attributing to Stoddard in his second period an *opus operatum* sacramentalism, ignoring Stoddard's own statements and Holifield's excellent delineation of Puritan sacramental theology. Interpretive positions on which Coffman draws (Miller is another example) are seldom acknowledged and never critically discussed.

One must conclude that this biography is not a reliable guide to Stoddard, either in its broader interpretations or in its factual details.

THOMAS A. SCHAFER
McCormick Theological Seminary

W. CLARK GILPIN, *The Millenarian Piety of Roger Williams*. Chicago: University of Chicago Press. 1979. Pp. viii, 214. \$17.00.

W. Clark Gilpin's volume continues the tradition, since Perry Miller, of treating Williams in terms of religious motivations. He argues that the unifying element of Williams's wide-ranging thought was his sense of religious vocation, interpreted within a distinctive "millenarian vision of history." Gilpin derives Williams's separatism from preoccupation with outward sanctification following conversion, while noting the similarity of his ideas to those of the General Baptists. He finds that Williams shared the widespread eschatological excitement of the Civil War period but distinguished himself from other Puritans and sectaries who found the millennial dawn in their own efforts to establish "pure" churches or spiritualized it as a personal transformation. For Williams, restoration of the true, visible

church had to await Christ's coming in the indefinite future. Until then, all churches are corrupt and all nations, civilized and savage, "heathen"; a Christian's proper calling is to prophetic "witness" against the corrupt mingling of spiritual and political realms.

These "Williamsian" notions are not unfamiliar. Gilpin, though, gives them coherent treatment, with attention to Williams's literary *corpus* and to controversial and religious currents that shaped them (though the latter occasionally crowds treatment of Williams himself). He demonstrates the pervasiveness of millenarian elements in Williams's thought, an important contribution. (Williams's millenarian consciousness, Gilpin maintains, emerged fully only after 1636, though it is not clear from evidence presented whether as the result of new influences on him or through development of motifs already present in his separatism.) He skirts the issue of Williams's relationship to contemporary "seekerism," a deficiency.

The force of the book is to suggest that Williams was fundamentally a millenarian. Yet this seems an inadequate impression of him as theologian and actor. His mature theology included a Reformed doctrine of nature and grace (shared with the Massachusetts divines), a perfectionist doctrine of sanctification (with radical implications for church membership), a restorationist doctrine of the church (with similar implications for church polity), a doctrine of Christ's spiritual lordship manifest in worldly weakness and humiliation (with implications for Christian discipleship and church-state relations), as well as a millenarian view of Christian history. Gilpin acknowledges this complexity but neglects Christology entirely, barely touches the issue of nature and grace, and tends to subordinate ecclesiology to eschatology. Williams's characteristic eschatological hope, however, seems the necessary reflex of his separatist-perfectionist ecclesiology, which constituted the abiding concern of his life. Gilpin says nearly as much (p. 159), yet his title and treatment imply that eschatology, as such, was dominant. The actions for which Williams is justly remembered, however, appear to have been motivated less by a longing for Christ's coming than by an earnest seeking after, and failing to find, the true church of Christ's institution.

WILLIAM K. B. STOEVER
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CECELIA TICHİ. *New World, New Earth: Environmental Reform in American Literature from the Puritans through Whitman*. New Haven, Conn.: Yale University Press. 1979. Pp. xii, 290. \$18.50.

A few years ago an advertisement for an American engineering company ran in many national maga-

zines. Two hard-hatted workers gazed across an alpine scene to a high snow peak; "O.K.," the text of the ad began, "where do you want it moved?"

The idea that Americans are divinely appointed and culturally prepared to move mountains, clear forests, drain swamps, and make the desert blossom like the rose is the subject of *New World, New Earth*. Cecilia Tichi's meticulous account does not extend to contemporary advertisements. She explores instead the roots of the concept that the wild New World environment must be reformed (literally reformed) so as to become the promised land of American millennial expectations. The New World would thus become the New Earth.

The genesis of this work appears to be the author's displeasure with the way the "ecology" movement of the 1960s altered the reputation of American pioneers. Instead of the heroes they were once held to be, frontiersmen, beginning with the Pilgrims and Puritans, were seen as ecologically blind exploiters who selfishly ravaged the virgin land. Unfair, responds Tichi. In their own minds at least the pioneers' alteration of the North American continent was a heaven-appointed task of world importance. In order for the perfection of man and society to proceed, the environment had to be redeemed from its wild condition. The transformation of the land, like the displacement of the Indian, was a work of God. Tichi's adept textual analysis traces these ideas in the work of Edward Johnson, Joel Barlow, and Walt Whitman, each of whom receive entire chapters. Figures of lesser importance, such as George Bancroft, and partial dissenters like Henry David Thoreau, are also well handled.

The revisionist element in *New World, New Earth* has to do with motives. Greed, fear, and hatred, says Tichi, took second place to idealism in explaining the drive of early Americans to vanquish the wilderness. Such reassessment is important. The 1960s was unfair in shaking the finger of moral reproach at previous generations. It is logical that the 1760s perception of such things as population growth, deforestation, and the extermination of wildlife would be radically different from that of contemporary Americans. Still one can not suppress an occasional suspicion that Tichi has been taken in by the rhetoric of her subjects. The idealism she describes might be viewed as intellectual oil to grease the wheels of a very materially oriented process. Just as Manifest Destiny rationalized the bullying of Indians and Mexicans, so the ideal of a new earth smoothed over anxieties concerning the wholesale alteration of the environment. As Bob Dylan recognized, Americans have been adept at putting God on their side.

RODERICK NASH
University of California,
Santa Barbara

DAVID HOROWITZ. *The First Frontier: The Indian Wars and America's Origins, 1607-1776*. New York: Simon and Schuster. 1978. Pp. 251. \$9.95.

The author of this slim, fast-paced volume believes that Americans have repressed the memory of their origins, which were above all else steeped in conflict with native occupiers of the land. To correct this historical amnesia, David Horowitz has written a breezy account of the first 150 years of American history, beginning with the Pilgrim arrival at Plymouth and culminating with independence. The book is largely narrative history. It details, usually with factual accuracy, the conflict with tribes in New England and Virginia in the founding years, the wars in 1675 that all but ended Indian resistance along the coastal plain in both the north and south, and the eighteenth-century wars for empire that left the powerful interior tribes bereft of a European ally to play off against the Anglo-Americans.

Considering the outpouring of work on Indian-white relations in the last fifteen years, one cannot claim that Horowitz has broken new ground. But it is possible that he will bring an interpretation that is well grounded in recent scholarship to the attention of those in the history-reading public who like their accounts of the past ungarished by footnotes and unencumbered by analysis.

GARY B. NASH
University of California,
Los Angeles

GARY M. WALTON and JAMES F. SHEPHERD. *The Economic Rise of Early America*. New York: Cambridge University Press. 1979. Pp. x, 226. Cloth \$19.95, paper \$4.95.

This survey sketches commercial developments, not general trends, in the British colonies of North America from the fifteenth-century background to 1792. Gary M. Walton and James F. Shepherd believe that foreign trade provides the key to understanding the economic growth that they feel occurred, and they think that recent research justifies a new synthesis, "a supplement to larger texts." They draw almost exclusively on books and articles by economic historians.

After a short introductory chapter, they take the reader quickly through the period of discovery and conquest, arguing for the critical importance of silver on the European economy. The next five chapters of one hundred and fifty pages contain the heart of the book and deal with colonization, the "ascent" of foreign trade, and economic "progress." A final fifty pages deal briefly with some economic aspects of the revolution and its results, stopping be-

fore the wars of the 1790s. Factually, the heart of the argument rests upon limited trade statistics during 1768-72 and 1791-92, which Walton and Shepherd have used in earlier publications. The authors insist that the colonists' wealth and standard of living grew, especially in the seventeenth century, due primarily to the commercial sector. They also emphasize increasing inequality as a result of commercialization.

A good editor would have improved the writing style and eliminated some errors. Among others, Irene "Niu" spells her name "Neu" (p. x), and it is "Bruchey" not "Bruckey" (p. 95). North America includes Mexico, so statements on pages 34 and 37 are false. All of the colonies south of Pennsylvania had resident merchants (p. 47). The statement about mortality rates (pp. 54-55) is not true of the coastal south. Freed indentured servants did not receive mules (p. 58). The British did not enforce energetically or effectively the acts of 1764-65 (p. 69). Pontiac's Rebellion did not precede but followed the Proclamation of 1763, a fact that changes its origin (pp. 162-63). The upper house in the colonies was not called "The Lords" (p. 154). The Sugar Act contained far more than a modification of the Molasses Act (p. 164). The Currency Act did not prohibit all paper money (p. 172).

Some historians will question certain interpretations. The emphasis on foreign commerce and commercial activities leads the authors to minimize the significance of agriculture and manufacturing and virtually to omit any description of colonial currency (for example, pp. 48, 88). As a result, the actual economic situation becomes distorted. Proof is needed that colonial mobility of labor and capital exceeded that in Europe (p. 115). The section on growth (pp. 140-42) is entirely speculative. The discussion of income and wealth rests upon very shaky foundations, and there is at least as much evidence on the other side: the authors present as fact what is only hypothetical.

In all fairness, the authors are not addressing historians but undergraduates. For students of economics who need some history, the book's limitations may not matter, and they will profit from its discussion of trade. History majors would require careful editing, broader coverage, and the incorporation of additional scholarly work.

JACKSON T. MAIN
State University of New York,
Stony Brook

JOHN PHILIP REID. *In a Rebellious Spirit: The Argument of Facts, the Liberty Riot, and the Coming of the American Revolution*. University Park: Pennsylvania State University Press. 1979. Pp. 168. \$12.95.

In a Rebellious Spirit is the latest in a series of monographs by John Phillip Reid dealing with legal conditions, forensic rhetoric, and political machinations in Massachusetts during the pre-Revolutionary decade. It focuses upon the legal and rhetorical skirmishing in Boston between British officials and Whig leaders that preceded the stationing of two regiments of troops in the town in October 1768.

In a point well taken, Reid argues that the letters, speeches, reports, and pamphlets issued by each side can only be properly understood within the framework of eighteenth-century English and colonial law and must be seen as persuasive documents designed not to record with strict objectivity and accuracy the state of public affairs but to impress upon officials in the mother country a particular interpretation of what was happening in Boston and what ought to be done about it. The aim of British officials—especially Governor Bernard and a clique of customs officers—was to portray Boston as a city utterly under the control of radical Whigs who had so reduced it to a state of lawlessness that the normal operations of government could not proceed without the protection of regular troops. Reid demonstrates how Bernard and the customs commissioners manipulated evidence, distorted events, and created “facts” in order to circumvent the Massachusetts constitution and persuade the ministry that soldiers must be sent to Boston.

Throughout the book Reid is highly critical of the motives and discourse of Bernard and the customs officers. So much so, in fact, that he tends to overlook the argumentative defects and exhortative excesses of the resistance movement. Reid’s sympathies clearly lie with the Boston Whigs, and he affirms their allegation that quartering troops in Boston was not really necessary to keep the peace and resulted instead from an assiduously executed plot by Bernard and the customs commissioners. Although Reid does not treat either the course of events in Boston from 1766 to 1768 or the motives of Bernard and the commissioners in their full complexity, his case for conspiracy is at least plausible and deserves attention at a time when much historical scholarship has become sympathetic with the Tory view of the Revolution.

Reid’s assumptions also warrant notice. He understands that the meaning of events—and their significance for the historian—rests, not in the events themselves, but in the perceptions of those people who participate in, observe, or respond to them. He also reminds us of the necessity of approaching the public documents of men like Bernard, Samuel Adams, and James Otis as what they were in their own time: rhetorical discourses designed to impose particular constructions of reality upon their readers. Such an approach is not new, of course, but the more it is employed, the better we

are likely to understand the essentially rhetorical origins of the American Revolution.

STEPHEN E. LUCAS
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Madison

DONALD WALLACE WHITE. *A Village at War: Chatham, New Jersey, and the American Revolution*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1979. Pp. 310. \$17.50.

The comparatively undistinguished village treated in this book has been the subject of half a dozen histories. Evidently Chathamites enjoy both reading and writing about their town. Donald Wallace White, a former resident and graduate student in history, was asked to write this book as a contribution to the bicentennial, although a general history had appeared in 1967.

The author chose as models recent New England town studies. He carefully combed through printed and manuscript state and local materials and prepared a study complete with scholarly apparatus. He identifies the 288 residents of Chatham during the 1775–83 period and in a series of tables summarizes data on the consequences of the Revolution for this hamlet. Some of the more intriguing points are the effect of the Revolution on marriages; the value of a newspaper in awakening the local people to a wider world; and the economic consequences, although the author weakens this section by confusing wealth with income.

Chatham was before 1806 divided politically among two counties and four townships; consequently, the political history during the Revolution is awkward to handle. White does not tell us if the Revolution encouraged unification of this locality. On political effects he seems contradictory, finding that after the Revolution “new Faces” came into a “remarkably free and democratic government” (p. 199), while representation and suffrage were “little affected” (p. 215).

White provides data on the religion (Presbyterian), occupations (mostly farmers), and economic status of the Chatham soldiers (pp. 66–67), though without enough attention to whether the Revolution was, as other studies indicate, a poor man’s fight. He should have explained the contribution of this locality in light of New Jersey’s low proportion of continentals raised and rather disorganized militia.

He might also have studied the general New Jersey circumstances carefully. That taxes in 1780 were incredible and impossible (p. 143) was not the view of the inhabitants; the percentage collected was relatively high. And a couple of minor points: no one had “to pay five hundred pounds of New Jersey

money" to obtain a marriage license (p. 158)—as a review of the clandestine marriage act would clarify; nor was Robert Ogden a Tory at the outbreak of the Revolution (p. 57).

Residents of Chatham should enjoy the book. Students and scholars will more profitably turn to Robert A. Gross's study of Concord for a depiction of the American Revolution in the small town.

BENJAMIN H. NEWCOMB
Texas Tech University

DONALD JACKSON and DOROTHY TWOHIG, editors. *The Diaries of George Washington*. Volume 3, 1771–75, 1780–81; volume 4, 1784–June 1786. Charlottesville: University Press of Virginia. 1978. Pp. xv, 494; xv, 405. \$20.00; \$15.00.

In March 1785 Washington referred to his work at Mount Vernon as his singular "amusement," which is what Donald Jackson and Dorothy Twohig continue to provide readers of Washington's diaries. These volumes stand in welcome contrast to the growing colorlessness that has become the hallmark of too many documentary editions. In the oftentimes admirable quest to eliminate undesirable editorial intrusion and to hold down costs, some editors have lost sight of the basic ingredients necessary to produce a good historical show. Jackson and Twohig have not.

With two notable exceptions in 1781 and 1785, the diaries remain terse to the point of being drab. The editors infuse the spare entries with a concoction of information. The result is a private narrative, as differentiated from an exchange of letters, made vivid by historical imaginations bent upon having Washington "come alive." The editors established early that the identification of individuals Washington named was of central importance. For the specialized or the general reader this information gives added human dimension to Washington's society, to his travels, and ultimately to his values. Whether he is meeting with his Pennsylvania tenants in 1784, Jean Antoine Houdon and Mrs. Catherine Macauley Graham in 1785, or Philadelphia's leading citizens in 1772, Washington resolutely refused to imbibe in the kind of personality sketching that titillates so many diary readers. Jackson and Twohig avoid the sin of editorial gossip and, unless there is clear evidence, do not postulate Washington's assessments of other people.

The other important annotation device involves the use of Washington's own correspondence as commentary for diary entries. Simple enough in conception, the task requires painstaking care in research and selection. Perhaps the most moving example appeared when Patsy Custis died, an event that brought forth only the briefest mention in the

diary and yet a loss that Washington's correspondence clearly shows as having moved him deeply. In a lighter vein, the editors' selection of various comments following the arrival of a jackass sent from the King of Spain highlight Washington's agricultural interests as well as his humor when he wondered if "Royal Gift" would respond to republican charms. The editors manage these selections in such an unobtrusive manner that the annotation becomes an almost integral part of the diary. Finally the editors utilize annotation to familiarize the reader with significant events. The drama of the spring and summer of 1774 would be lost without the notes, as would the significance of the day Congress voted to install Washington as Commander-in-Chief. The editors resort only once to a headnote, that increasingly popular expression of editorial virtuosity. Add to this the editors' steadfast and courteous refusal to exploit annotation for historiographic pyrotechnics, and a rather full-blown annotation policy for Washington's diary becomes clear. Presumably each reader will find certain areas of annotation either lacking or excessive. Given my pack-rat proclivities, even more illustrations, profuse and useful as they are, would be welcome. Of greater consequence, the maps in the first two volumes can be used in conjunction with these volumes, even though the editors make no references.

Inevitably questions are raised regarding the historiographic thrust of annotation, particularly when it occupies a central place as it does here. Jackson and Twohig, following the natural lead of the material, accentuate Washington's private side—family relationships, friendships, social and cultural concerns, and his sense of the land. Absent are discursive comments about Washington's military leadership, his financial interests in opening navigation to the Ohio, and the resumption of his political interests after the war. Throughout these volumes there is an implicit caveat against inductive leaps. The result is a delight for readers, regardless of their interests. In a time when the function of professional historians is increasingly called into question, these volumes stand as apt illustration of the assertion that competent professionalism can produce a strikingly evocative view of the past, in this instance, Washington during his most energetic years.

GEORGE M. CURTIS, III
Papers of John Marshall

BURTON SPIVAK. *Jefferson's English Crisis: Commerce, Embargo, and the Republican Revolution*. Charlottesville: University Press of Virginia. 1979. Pp. xiii, 250. \$17.50.

Thomas Jefferson's two terms as President present a striking contrast. The first four years, marked by

domestic policy successes and the purchase of Louisiana, culminated in a re-election victory that nearly crushed the opposition party. The second term was filled with mounting disappointment and capped by costly failure. The embargo shattered hopes of harmony between the sections and revitalized the opposition. Its repeal demoralized the president, weakened his successor, and suggested that the warring European empires could continue to pursue restrictive policies without provoking an effective American response.

This volume seeks the sources of Jefferson's failure either to avoid or to resolve the crisis in Anglo-American relations that developed after 1803 with the renewal of the Napoleonic wars. The emphasis is on the changing course of the administration's English policy as it was influenced by political considerations, personality, and—most especially—republican assumptions and ideals. As Burton Spivak sees it, the United States was lured into a full-blown crisis by the administration's determination to preserve the wartime carrying trade, which was both highly lucrative and crucial to the effort to conciliate New England even if at odds with some of the most basic Jeffersonian beliefs about a sound republican society. Strained by the effort to protect a kind of commerce he had never thoroughly approved and readily convinced of the malevolence of British policy, Jefferson was ready to resort to war in the aftermath of the *Chesapeake* affair. But his decision not to call an early session of the Congress prevented preparations and cooled the country's anger. When Congress did convene, new French and British edicts posed a danger that was met by the embargo.

Spivak argues that, for Jefferson himself, the embargo was originally intended as a temporary measure, a traditional preface to hostilities. Only gradually did Jefferson accept James Madison's conception of an indefinite embargo as a method of coercing France and Britain. As he did, however, he increasingly envisioned the embargo as an agency for shaping a healthier republican economy and as a test of the success of the republican polity. Confronted by its failure in his final weeks in office, he could neither give it up nor bring himself to recommend a war.

Non-specialists will find this study difficult to follow. It is not well organized or written. Specialists are likely to resist the most revisionary aspects of the argument. I think it over-emphasizes the role of the defense of the re-export trade and simplifies the progress of Jefferson's thought, while making him too ready to resort to war. Yet *Jefferson's English Crisis* usefully explains the way in which the Jeffersonians combined (perhaps confused) two different policy traditions—non-intercourse as peacetime method of coercion and embargo as defensive pref-

ace to a war—and Spivak offers a revealing illustration of the complexity of Republican thought and practice.

LANCE BANNING
University of Kentucky

PHILIP L. WHITE. *Beekmantown, New York: Forest Frontier to Farm Community*. Austin: University of Texas Press. 1979. Pp. xv, 378. \$24.95.

This is a detailed description and analysis of a Lake Champlain frontier community over two generations, extending roughly from 1790 to 1850. Lack of relevant data precludes a demographical analysis, yet on the basis of tax records, decennial federal census returns, and other material, Philip L. White poses several provocative hypotheses on the influence of the heavily forested frontier. The economic advantages of lumbering and related industries attracted a first generation of settlers relatively high in socioeconomic status. The original proprietors, the Beekman family, failing to dispose of their tract in large blocks for a quick profit, decided to sell or lease land in farm-lot pieces to individual settlers. Competition from other proprietors forced the Beekmans to be reasonable, indeed, even compelled them to accept violations by settlers of the terms of their agreements. Many of the earlier settlers were businessmen and entrepreneurs as well as laborers and farmers.

Through the first generation of settlement the marketing of forest products sustained an expansive, prosperous economy. From the mid-1820s on, approaching exhaustion of marketable timber sharply curtailed prosperity, altered the economy, and drove the upwardly mobile, those with resources, to move. The men who remained in Beekmantown evidently lacked capital for a move west or the experience necessary to take advantage of opportunities elsewhere. While relatively poor French Canadians and Irish Catholics moved into the region about Lake Champlain, immigrants from New England and other areas in New York seemed to prefer the more rewarding opportunities offered farther west where land was cheaper, more likely to rise in value, and more productive. The result of these shifts in population and the exhaustion of the forest frontier was evident in the census of 1850. Land in Beekmantown was less valuable than land elsewhere in the state. The size of farms was "considerable"—White's term—but relatively few farms were either very small or very large. The twenty poorest farmers held from 6 to 50 acres, the wealthiest from 90 to 250. Only four held over 177 acres of improved land. Disparities in the size of individual land holdings were much less than during the first generation.

Not surprisingly, the dominant political loyalties of the region were Jeffersonian Republican and Jacksonian Democrat, reflecting a provincial backwater rather than cosmopolitan community. The economic lot of the common people improved, White concludes, despite the economic decline. Beekmantown was a more egalitarian society. One wonders if this might not have been an equality of near poverty. Unfortunately, the hard evidence on which to base these conclusions depends on early tax records and the federal census. How reliable are these? How typical was Beekmantown, Clinton County, and the Lake Champlain frontier? Other frontier regions less dependent on timber and blessed with soil more conducive to agriculture might show a different pattern.

J. M. SOSIN
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Lincoln

HOWARD B. ROCK. *Artisans of the New Republic: The Tradesmen of New York City in the Age of Jefferson*. New York: New York University Press. 1979. Pp. xviii, 340. \$17.50.

This is an exceedingly good and useful book. Howard B. Rock has studied New York's mechanics during the early national period (1801-19) with care and clarity. The research is thorough, the analysis is balanced, the presentation is effective. Rock provides an excellent microcosmic elaboration and undergirding for Alfred Young's seminal *Democratic-Republicans of New York* (1967).

New York's early national mechanics participated in partisan politics as a means to advance their status in the community. Their general preference among national political parties was for the Jeffersonian Republicans, a preference based on both ideological and practical considerations. The Federalists' elitist leanings repelled the artisans, but practical, local political issues related to conflicting party ideals were equally influential in determining their votes. The Republicans represented opportunity for the common man—opportunity to participate in society and government as the equals of the gentry and to leave behind the deference of the eighteenth century, opportunity to effect suffrage reform, opportunity to hold significant political offices, and opportunity to support social and economic policies that advanced their well-being. The choices were not always easy: during the Embargo and the War of 1812 the tradesmen had a difficult choice to make "between principle and pocketbook" (p. 89), and frequently local issues and local politicians presented problems that disillusioned the urban working class. Yet Rock's detailed analysis never loses sight of the long-range commitments.

It is clear that democracy for the common man in New York did not have to await the advent of the Age of Jackson.

Rock's analysis of economic history is, if anything, more perceptive than his political history. He finds great diversity in the artisan community, not only between masters and journeymen but also between those who adopted emerging modern techniques of production and marketing and those who did not. For the artisan, "the central meaning of the Revolution, together with the fulfillment of republican ideals, was the opportunity for unfettered entrance into the marketplace" (p. 152)—an opportunity they explored and advanced aggressively in the early nineteenth century. They borrowed corporate business models from merchants, they experimented with modern capitalist credit schemes, they organized to support tariffs, they both established monopolies and fought against monopolies, they campaigned for favorable governmental policies, they caused labor unrest and used primitive collective techniques, and they even developed a new "work culture and moral outlook" (p. 295). Rock shows that these developments had roots in a struggle for economic leverage, but he is equally clear that the mechanic was not a mere pawn of his own economic striving: "standing in society" and "striving against deferential expectations and contemptuous treatment" (p. 279) were just as important to him.

Rock's book will be a standard reference for a long time. Its analysis is complete, it uses both traditional and novel source material, it balances modern quantitative methods with literary, biographical, and visual analysis, and it argues the case for the new social history without rejecting the considerable contributions made to our knowledge by the old.

CHARLES S. OLTON
Barnard College

WILLIAM R. BROCK. *Parties and Political Conscience: American Dilemmas, 1840-1850*. (KTO Studies in American History.) Millwood, N.Y.: KTO Press. 1979. Pp. xvi, 367. \$20.95.

Parties and Political Conscience may be the best single-volume history of national politics in the 1840s ever written. It is graced by acute portraits of major personalities, lucid dissection of issues and opinion, and brilliant comparative insights into the ways the American constitutional system influenced the policy-making process and the workings of political parties.

William R. Brock is concerned with the collapse of the second party system, and his thesis hinges on the centrality in mass political behavior of moral attitudes, or "political conscience." The intense loy-

alty American voters developed toward the Whig and Democratic parties by 1840, he argues, was rooted in differing interpretations of the nation's moral purpose and in a belief that the rival parties offered alternative ways to effect it. A series of developments during the 1840s—the failure of the Whig majority to enact its economic program in 1841–42, the timid inflexibility of both parties when confronted with changing social and economic conditions, the outbreak of the Mexican War, and the virtual cessation of policy output by decade's end—eroded voters' confidence that the parties could meet their moral expectations.

Above all else, Brock contends, the emergence of the slavery extension issue raised fears that the nation's moral purpose was threatened and turned men against the parties. As Northerners and Southerners became convinced that their respective views of the proper moral order could be obtained only through the prohibition or achievement of slavery expansion, more and more of them prepared to jettison the national parties as useless. Whereas most historians locate this decision in the 1850s, Brock insists that disillusionment developed earlier. Events between 1844 and 1846 formed a critical turning point from which the parties could never recover. By 1848, “most thinking and humane men” (p. 153) correctly regarded the party system as bankrupt. The Compromise of 1850 only confirmed their suspicions, he asserts, because it sacrificed political conscience for expediency.

Although much of Brock's analysis is thus a forceful reassertion of the primacy of the slavery issue in destroying the second party system, historians will find much that is fresh and worthy of serious attention here. Certainly his imaginative attempt to link so many disparate developments as solvents of public confidence in the party system merits applause. Yet one can agree that such a loss of confidence ultimately destroyed the system without agreeing that it occurred as early as he says or that antagonistic sectional attitudes toward slavery were as disproportionately important in causing it as he asserts. For one thing, many of his boldest generalizations about public opinion lack evidence and are debatable. For another, he exaggerates the weakness of the major parties with the electorate in the 1840s by inflating the importance of atypical dissidents from the system like Conscience Whigs, Barnburners, and intellectuals. Finally, his narrow focus on Congress and national politics blinds him to the role that state-level political conflict played in reinforcing voter loyalty throughout the 1840s. Despite the vigorous arguments of this important book, in short, a case can still be made that it was only in the 1850s, when the old parties were viewed as useless at all levels of the federal system and not just by

men who gave priority to the slavery issue, that the system collapsed.

MICHAEL F. HOLT
University of Virginia

TAYLOR STOEHR. *Nay-Saying in Concord: Emerson, Alcott, and Thoreau*. Hamden, Conn.: Archon Books. 1979. Pp. 179. \$14.50.

Recently, the best books on the Concord Transcendentalists as a group (I'm thinking primarily of Lawrence Buell's *Literary Transcendentalism*) have concentrated on their strategies as writers. Only biographies and studies of individual figures have really examined their thoughts and deeds.

Taylor Stoehr, author of *Hawthorne's Mad Scientists* and editor and authorized biographer of Paul Goodman, takes the rather old-fashioned view that “The transcendentalists' first question was not how to write or speak, but how to live” (p. 149). Even more old-fashioned, perhaps, he seeks the meaning of their lives for our own, as leaders of an earlier “counterculture movement” against injustice, alienation, and commercialism.

The focus, therefore, is on the three general issues in the lives of Emerson, Alcott, and Thoreau that Stoehr thinks most relevant to the counterculture of the 1960s as it survives today. Stoehr's choices are civil disobedience and the nonpayment of taxes; utopian communities and attitudes towards division of labor, property, and family; and, finally, “Food for Thought,” or the relation of vegetarianism and dietary self-denial to introspection and the “new consciousness”—a phrase Emerson used in “Historic Notes of Life and Letters in New England” (p. 119). By comparing the different positions that these three men took on these issues, Stoehr manages not only to make their experiences instructive but also to give us sharp and sometimes humorous insight into their characters. Taking his evidence from journals, letters, and early memoirs (and almost ignoring modern biographies and criticism), he rediscovers the freshness and limpidity of their original behavior.

What Stoehr finds that they had in common is perhaps the most important message of the book. Their achievements, he feels, were all made “by virtue of fierce self-discipline and restraint.” They drew back and revolted against “a world of *too much*”—“too many careers, too many possessions and pleasures, too much complexity and ramification, too much leisure in which to think about all these alternatives, and to dream up more” (p. 155). Their nay-saying, therefore, was not Melville's cosmic “NO! in thunder” or the sense of history and tragedy through which Hawthorne viewed Ameri-

can optimism. It was more fundamental. It was conservative self-denial and voluntary abnegation from a world going crazy over getting and spending.

This reading of Emerson, Alcott, and Thoreau should alarm anyone who still sees them as soft-headed idealists and thinks that there is greater virtue in Hawthorne's, Melville's, and Poe's "Power of Blackness." To call the Transcendentalists "naysayers" is indeed a radical countermove in the old critical debate. Yet Stoehr does not make it just for effect or from infatuation with the modern counterculture. On the contrary, he argues that modern students desperately need the Transcendentalists' lesson of "refusals and abstinence."

This is a challenging, timely book.

ROBERT F. SAYRE
University of Iowa

HERMAN BELZ. *Emancipation and Equal Rights: Politics and Constitutionalism in the Civil War Era*. (Norton Essays in American History.) New York: W. W. Norton. 1978. Pp. xviii, 171. Cloth \$10.95, paper \$3.95.

Without any doubt the author of this slim volume has fulfilled with distinction the purpose of the Norton Essays in American History. The continuing series edited by Harold M. Hyman seeks to provide concise interpretations of certain aspects of American History.

There is no pretence that this is an original research contribution. As an interpretive study, however, it is based on such research completed by the author in earlier works and on the very real contributions to knowledge by recognized scholars in the field of the Civil War and Reconstruction. Footnotes indicate specific indebtedness. A bibliography and index are especially useful to those not already acquainted with the period and topic.

Conclusions are clearly stated. Herman Belz contends that truly "significant nationalizing changes" (p. 142) did take place as a result of Republican reconstruction and that they maintained the essence of a federal state with state rights that conformed to the basic concepts of nineteenth-century thought. In these respects the author claims that reconstruction was successful not only in restoring the Union but also in confirming the emancipation of former slaves with equal legal rights in a free society. He recognizes the purpose of political expediency that prompted many acts but argues that they were no less just because of mixed motives.

Although recognizing that many achievements in reconstruction were only temporary and inadequate, the author contends that they formed the essential bases of greater results in the twentieth

century. According to this interpretation, Republicans succeeded rather than failed because the increment of change was of great and permanent significance. Racial prejudice existed but was diminished by events of the Civil War era. As measured by nineteenth-century conditions and outlooks, progress was made despite the apparent failure of much that was then initiated. Even "segregation was a decided gain" (p. 147) as compared with treatment of blacks before the Civil War.

Readers are warned against judging events and results of the period by modern standards. In the context of the 1860s a large impact was apparent. Thus the success of the Republican program is declared to be more than simply foundation for the future.

This interpretive work is literally crammed with facts, but they do not interfere with a casual reading. The secret seems to lie in a smooth and clear style that uses the mass of detail to contribute in an interesting way to the interpretation. Because it is an interpretive study, there will be those who disagree with certain conclusions, but none can deny the careful marshaling of facts to support the author's views.

Probably because of Belz's earlier studies of the Civil War era, he has devoted a disproportionate share of this work to the war years, when theory and basic attitudes toward reconstruction were evolving. Nevertheless, the postwar years are adequately treated as related to his purpose. Criticisms such as this and certain questions of the complete accuracy of some statements may be raised, but they are few. In different ways the book is interesting and useful to both layman and scholar.

JOSEPH B. JAMES
Wesleyan College

PATRICK W. RIDDLEBERGER. *1866: The Critical Year Revisited*. Carbondale: Southern Illinois University Press. 1979. Pp. xiii, 287. \$18.95.

Almost fifty years ago, Howard K. Beale published *The Critical Year: A Study of Andrew Johnson and Reconstruction*. A well-written indictment of radical policies, the book sought to demonstrate that Andrew Johnson's defeat was due to his failure to exploit the alleged underlying economic differences between the radicals and their opponents and that racial problems were only of secondary importance. Since that time, many revisionist writers, inspired in part by Beale's own call for a re-evaluation of Reconstruction historiography, have not only refuted most of his contentions but reasserted the justice and sincerity of the radical cause. Now Patrick W. Riddleberger, the biographer of George W. Julian, has

sought to re-examine Beale's thesis in a new book, 1866: *The Critical Year Revisited*.

In general, the author has succeeded very well. Reviewing the political and economic history of the period, he tends to reinforce the findings of the revisionists, especially those of Stanley Coben, Eric L. McKittrick, and LaWanda and John H. Cox. Thus he once again emphasizes the importance of the political and racial impact of Johnson's policies. His expertise in tracing the development of presidential Reconstruction, its threat to the supremacy of the Republican Party, and its inevitable alienation of the moderates, is commendable. A detailed analysis of the Civil Rights Act and the Fourteenth Amendment, as well as a meticulous account of the Memphis and New Orleans riots, serve to clarify the growing rift between president and Congress.

In view of the time that has passed since Beale first wrote his volume and the many revisionist works that have appeared since, the present study serves as a convenient summation of modern scholarship. If it adds little that is new, it nevertheless re-emphasizes the total inadequacy of Johnson's policy. As Riddleberger correctly reminds us, Abraham Lincoln, far from being as interested in speedy restoration as his successor, was infinitely more flexible and had long been moving toward his radical critics. The author also points out that the lengthy debates concerning the Fourteenth Amendment showed that the Civil War did not result in as much centralization as is usually thought.

What Riddleberger fails to explain is the reason for Johnson's refusal to come to terms with the moderates. That he might have prevailed by cooperating with such congressional leaders as Lyman Trumbull and William Pitt Fessenden is evident, but in view of his determination to keep the South "a white man's country" he could hardly be expected to do so. It was precisely this determination that led him to defy such moderate proposals as the Freedmen's Bureau Bill and the amendment. Only after he had succeeded in thoroughly undermining Reconstruction did he finally enter into compromises with the moderates, making agreements that resulted in his acquittal in the impeachment trial. But by that time he had already encouraged southern conservatives to such a degree that he contributed greatly toward the ultimate triumph of reaction.

Notwithstanding this oversight, the book is a valuable addition to the historiography of Reconstruction. Its lively style and attractive format ought to guarantee it the wide readership it deserves.

HANS L. TREFOUSSE

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ROBERT F. HOROWITZ. *The Great Impeacher: A Political Biography of James M. Ashley*. New York: Brooklyn

College Press; distributed by Columbia University Press, New York. 1979. Pp. xii, 227. \$15.00.

The subtitle of this volume accurately describes its contents, for it is primarily a narrative account of James Ashley's political career with heavy emphasis on his years in the House of Representatives. Since the only other biography of this controversial and enigmatic figure was written by the congressman's son and published in 1907, this book fills some long-standing gaps.

The author has two major objectives in *The Great Impeacher*. First, his title notwithstanding, Robert F. Horowitz wants to demonstrate that Ashley should be remembered for his significant role in the history of Radical Republicanism writ large, not merely as the earliest and most aggressive Congressional advocate of impeaching Andrew Johnson. Second, Horowitz is out to defend Ashley against the charge of being "a nut with an *idée fixe*" (as C. Van Woodward phrased it) on the subject of trying to remove the president.

Horowitz is reasonably successful in meeting his first goal. The sections on Ashley's espousal of black rights, on Ashley as a political realist driving the Thirteenth Amendment through Congress, and on Ashley as a pioneer in the formulation of reconstruction policy are the strongest in the book. Horowitz is less successful in defense of Ashley the impeacher. Early in the biography Horowitz hints at a psychohistorical explanation of Ashley's patricidal behavior, but the hints are not followed up. Horowitz is able to document Ashley's longstanding fear of executive tyranny, but that alone, however fashionable in the 1970s, hardly justified a course in the 1860s that embarrassed even Ashley's closest ideological allies. When the successful impeachment finally began, Ashley's fellow Republicans tried to freeze him out of a leadership position in the proceeding, and the Ohio State Republican Committee refused to help him run for re-election in 1868. Since Ashley almost won that race anyway, there may be a case for the Toledo Congressman's actions after all. But the case Horowitz makes is inconsistent and apologetic.

Because Ashley's private papers burned during his lifetime, Horowitz has relied primarily on the public record, newspapers, other histories, an autobiographical memoir written late in Ashley's life, and letters culled from collateral manuscript collections. His research, especially in the collateral collections, is impressive, and he deserves credit for uncovering as much as he has. Nonetheless, James Ashley, a maverick even by the standards of the most tumultuous period of American history, remains a controversial and enigmatic figure.

JAMES C. MOHR

University of Maryland,
Baltimore County

WILLIAM C. HARRIS. *The Day of the Carpetbagger: Republican Reconstruction in Mississippi*. Baton Rouge: Louisiana State University Press. 1979. Pp. xiv, 760. \$37.00.

James W. Garner's *Reconstruction in Mississippi* (1901) has long been considered not only the standard treatment of the subject but also the best of the state studies by members of the "Dunning school." In one book Garner covered the Civil War and both presidential and congressional reconstruction. Now William C. Harris, having already produced *Presidential Reconstruction in Mississippi* (1967), devotes the whole of a huge volume to congressional reconstruction in the state. His two volumes constitute a prodigious work, much the most circumstantial account of statewide reconstruction anywhere in the South. To the period 1867-1876 he gives three times as much space as Garner gave. One may question, however, whether he adds proportionally to historical truth.

Harris has "not attempted a detailed account of the social and cultural history of blacks," he explains (p. xii), but has "sought to weave black activities into the general story." In telling the general story, he devotes separate chapters to education, economic developments, and "country and town life," including such sidelights as the rising popularity of baseball after its introduction by Union troops. He shows that, in the larger towns, "Republicans and conservatives, blacks and whites, northerners and natives" were "in favor of economic growth and the construction of railroads" (p. 286). He deals with issues that arose independently of the reconstruction question—and that also agitated contemporary Wisconsin and other northern states—such as the promotion of railroads for the exploitation of timber, the use of school lands to subsidize them, railroad regulation, temperance, compulsory school attendance, prisons and the convict lease system, and pauper relief. In his excellent handling of these topics, he makes his most valuable contribution, going far beyond Garner.

In recounting politics Harris relies very heavily on Democratic or conservative newspapers, especially the *Jackson Clarion* of Ethelbert Barksdale, whom he describes as "the most vigorous foe of the 'mongrelization' of the state" (pp. 131-32). He quotes a carpetbag editor who justified the use of falsehood in a good cause, but he seems oblivious of the Democratic and conservative editors' widespread resort to the tactic of the big lie. He even cites the self-serving history by John S. McNeily, one of the most rabid of all the white supremacists. On the other hand, he slights or ignores the testimony of regular Republicans, in particular the testimony they gave before the congressional committee investigating the 1875 election.

Quoting and paraphrasing conservative spokes-

men at length, and adopting their words and their views as his own, Harris makes the carpetbagger Adelbert Ames and the Ames Republicans the villains of the Mississippi story. The "opportunistic" Ames had an "almost paranoid attitude toward 'rebel democrats'" (p. 54) and in 1869 "played the role of military despot" (p. 253). His career reveals the "intermingling of political ambition and wartime idealism" (p. 93). He "opened wide the door of opportunity for adventures and scamps," for "soldiers of fortune" (p. 54). By his "ineptness" and provocation he exacerbated race conflict, and he and his followers were to blame for the ultimate failure of the Mississippi experiment in biracial democracy. So Harris charges, while sparing Barksdale, McNeily, J. Z. George, L. Q. C. Lamar, and the rest of the reactionaries. He does not question their motivation, nor does he apply any pejoratives to them.

Throughout, Harris minimizes the role of violence in Mississippi politics. He says that Mississippians had always been violence-prone and that most of the mayhem and murder during the reconstruction period was nonpolitical. Here, again, he follows the conservative line of that time. In regard to the Ku Klux Klan, he disagrees with Garner, Vernon L. Wharton, and Allen W. Trelease, who have maintained that one of the Klan's basic aims was the destruction of schools for blacks. Harris admits (p. 328) that there were "attacks on obnoxious teachers" but contends that the Klan directed its hostility against the teachers' "political and social activities" more than "their work in establishing free schools," as if the two kinds of activity could, in the circumstances, have been separated. He also explains the rise of Ku Kluxism, if he does not justify it, on the grounds that anti-Republican counties were "saddled with a slate of obnoxious appointed officials" who were "in disrepute" and hence incapable of suppressing crime (p. 371).

Finally, playing down both the role of violence and the issue of white supremacy, Harris emphasizes the "failure" of Republican leadership and the economic grievances of white landowners in the 1875 overthrow of the Republicans. This he sees as essentially the culmination of a "taxpayers' movement." Here he accepts what elsewhere (p. 375) he himself characterizes as "extreme anti-Radical propaganda, which was only partly believed by the conservative leadership." Garner, no advocate of equal rights, was frank to say "a superior race will not submit to the government of an inferior one" (Garner, p. 408). Harris, by contrast, does subscribe to the principle of racial equality—as did the Mississippi conservatives in their 1875 platform.

RICHARD N. CURRENT
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DWIGHT B. BILLINGS, JR. *Planters and the Making of a "New South": Class, Politics, and Development in North Carolina, 1865-1900*. Chapel Hill: University of North Carolina Press. 1979. Pp. xiii, 284.

In an analysis patterned on Barrington Moore's *Social Origins of Dictatorship and Democracy*, sociology professor Dwight B. Billings, Jr., challenges C. Vann Woodward's interpretation of the post-Civil War eclipse of southern planters. Moore projected the model of a "Prussian Road" to modernization forged by industrialists and landed elites allied against lower classes. Presumably southern modernization fits this pattern, preserving and intensifying authority and repression. Unfortunately, this work contains more interpretation than primary research. One manuscript collection is cited followed by thirteen pages of published works, theses, and dissertations. With considerable understatement, the author warns: "My goal . . . is as much to develop hypotheses about the South as to test them."

The work's major contribution is its contention that the planter elite dominated the New South. Mini-biographies illustrate planter persistence in North Carolina's power structure, are based on solid historical research, and should be taken seriously. When the author moves from analyzing data to interpretation, however, he encounters problems. His analysis of Populism and Progressivism are examples. Following other historians of Populism, Billings noted that many Populist leaders were from the landed class and charges that the Populist refusal to engage in "an honest treatment of southern stratification" was "a key factor in their political demise" (p. 190). But Billings himself demonstrates that some of these landed "aristocrats" became radical Populists. Their failure was due less to internal restraints than to external forces: whatever effort Populists made at biracial economic radicalism was fastened upon by conservative Democrats and used to bludgeon them into submission. Billings' notion that "genuine class consciousness in the rural South was an awesome possibility" presumes that more radical Populism would have been successful, a dubious proposition. The author also concludes that Progressive educational reforms actually were "profoundly conservative." But this is a theorist's definition of "progressive" that does inadequate justice to a different age, which believed that schools and compulsory attendance laws would end illiteracy, child labor, and poverty.

The author's use of comparative history also is strained. The race factor makes it difficult to compare modernization in Germany, Japan, and the American South. Japanese cultural patterns, which Ruth Benedict described in *The Chrysanthemum and the Sword* (1946), including confidence in hierarchy

and the industrial paternalism resulting from *giri*, seem quite remote from North Carolina's mill villages and plantations.

Billings's hypotheses also must be applied cautiously to other southern states. Jonathan Wiener's *Social Origins of the New South* preceded Billings's work by a year. Wiener, one of Moore's students, applied the "Prussian Road" thesis to Alabama, the state that Billings uses for many of his comparisons. Wiener, like Billings, found remarkable planter persistence. But where Billings emphasized upper-class consensus after 1865, Wiener discovered fierce conflict between planters on one side and New South merchants and industrialists on the other. Whereas many Carolina planters apparently endorsed industrialism, Alabama planters opposed "the growth of a genuinely industrial society in their midst" (Wiener, p. 162). Although sharing Moore's interpretive assumptions with Billings, Wiener believes that Alabama Populism was "a period of decisive class conflict in Alabama; the Populist uprising was the first organized threat to planter rule since . . . secession" (Wiener, p. 222).

Studies of five Alabama counties (Wiener) or one southern state (Billings) should have yielded more temperate claims; a great deal more basic research needs to be done before one of Moore's devotees treats us to a Dixie-bred Bismarck.

WAYNE FLYNT
Auburn University

PAUL D. ESCOTT. *Slavery Remembered: A Record of Twentieth-Century Slave Narratives*. Chapel Hill: University of North Carolina Press. 1979. Pp. xv, 221. Cloth \$12.50, paper \$7.00.

New black militancy has led to a transformation of the historiography of American slavery. We have had a new literature heavily based on antebellum slave narratives and upon twentieth-century interviews with former slaves. The largest part of this material is the former slave narratives collected by the Federal Writers' Project of the nineteenth-thirties.

Paul D. Escott has submitted an important part of the Federal Writers' Project materials, as well as two volumes produced at Fisk University, to an exhaustive statistical survey and has written a straightforward work that affirms much of the recent work on American slavery based on the slave narratives. Among Escott's conclusions about slavery based on his examination of the narratives are; an overwhelming majority of former slaves saw their masters as "pursuing a selfish advantage" (p. 24); a great majority reported a personal experience with whippings or beatings; hard work was nearly a universal lament; food, clothing, and shelter were

simple but adequate in quantity; terrible physical abuse often did occur; roughly one-fifth reported at least partial breakup of their families under slavery due to sales.

Escott argues that under slavery there was an autonomous and viable slave community and culture, although slavery was a harsh and heavily exploitative social system. Moreover, he devotes considerable space to a partial refutation of Eugene D. Genovese's view that the slave narratives indicate the masters and slaves lived in the same world on the plantation, a world over which the masters had "hegemony" and exercised paternalistic control. Escott says that "the narratives make clear that masters and slaves lived in different worlds . . ." (p. 20) and that "paternalism related more to talk about the plantation than to what actually went on there" (p. 20). One might wish that Escott had been more explicit about the use of the narratives by other authors.

One weakness in the book is that a list of the interviewers of the former slaves for the Federal Writers' Project by state and race based upon his evaluations after reading the narratives is inaccurate. An examination of government personnel records would have provided a more accurate list. Some of those listed as white were black; some of those listed as black were white; for almost all of the many listed as "race unidentified" the personnel records supply the data.

Nevertheless, this is a useful, intelligent, readable book, which presents the findings of a most important statistical survey and clears the way for further, more detailed work utilizing the slave narratives.

GEORGE P. RAWICK
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MARTIN KAUFMAN. *The University of Vermont College of Medicine*. Hanover, N.H.: University of Vermont College of Medicine; distributed by University Press of New England. 1979. Pp. xviii, 275. \$15.00.

This is a detailed account of the "country" medical school that was founded in Burlington in 1804 and that, although it was dormant from 1836 to 1854, unlike most such schools, has managed to continue in existence. It became affiliated with the University of Vermont in 1821. The book is based on careful research in the archives of the college and tells the story as it was, with virtually no embellishment.

The value of the book lies in its being a case history of medical education in the United States. It describes the personal egotisms, the provincialism, the financial motivations and manipulations, the institutional competition (not always gentlemanly),

the inadequacies of the didactic lecture and private preceptor system of medical education, and the rural-urban school debate of the early nineteenth century. None of this was particularly peculiar to Vermont.

The nineteenth century saw constant attempts to reform medical education. At Vermont many of these attempts emanated from the college itself, but most came from pressures from the Carnegie Foundation, the American Medical Association, and the Association of American Medical Colleges. Often on the fringe of loss of accreditation, the college continued in existence. The ability of the college to withstand these pressures—including that from the Flexner report early in the present century—was nothing short of phenomenal. Recommendations to go out of existence or to limit the college to a two-year program were steadfastly resisted by the college. Yet there is ample evidence of the important influence of these national agencies in shaping American medical education.

The problems of the college regarding full-time versus part-time faculty, clinical resources, and relationships with the local hospitals were—and are—not unique to Vermont. The kind of town-gown difficulties to which medical schools without their own clinical facilities were prone pervaded the history of the college from the late nineteenth century on. Other problems of medical education—the budgetary competition of medical colleges with other colleges of the university, the relationships between preclinical and clinical faculties, the premedical requirements, the length of the medical program, the constant improvement of the curriculum, and the impact of federal support for medical education—are also presented in this account. The tenacity of the college and its ability to support medical education in a rural state-supported situation gives it importance in the history of medical education. It is interesting, too, that the state of Vermont boasts one of the highest ratios of physicians to population.

The book presents little analytical use of the data but touches upon some of the broader aspects of the history of medical education (the impact of the biomedical sciences and the responsibility of the medical education establishment in meeting the social needs for health care, for example). Yet it is spared from becoming an exercise in parochial chronology by the author's familiarity—he has also written *Homeopathy in America* and *American Medical Education: The Formative Years, 1765–1910*—with the broader aspects of the history of the nation's medical institutions.

An unusual feature of the book is that it has been composed with extrawide margins and that the notes have been placed in the margins. This resort to an ancient usage proved facile and pleasing and

superior to notes at the foot of the page, and, most certainly, to notes at the end of a book.

DAVID L. COWEN
Rutgers University

ELIZABETH ALDEN GREEN. *Mary Lyon and Mount Holyoke: Opening the Gates*. Hanover, N.H.: University Press of New England. 1979. Pp. xvii, 406. \$17.50.

In 1837 Mary Lyon founded in South Hadley, Massachusetts, the Mount Holyoke Female Seminary, which was destined to become the first permanent institution in our country for the higher education of women. In this scholarly volume Elizabeth Alden Green gives us both a biography of Lyon and the history of the first twelve years of the seminary she established. As emeritus professor of English and for twenty-five years director of the news bureau at Mount Holyoke, the author brings a rich background to her writing.

Mary Lyon was born in 1797 to a farm family in Buckland in western Massachusetts. What distinguished her from the other young women in the community were her eagerness and determination to secure an education. In 1814, recommended by one of her teachers for a teaching post, Mary began a life-long career in education. Compensation in her first position, "a summer term" of twenty weeks, was seventy-five cents a week and board among the patrons. As a teacher Mary improved her own education by studying at available institutions, notably coeducational Amherst Academy and Byfield Seminary for young women. At Amherst in 1818 she formed friendships that would give her the closest associations at Amherst College after it was chartered in 1825. At Byfield she was inspired by the teaching of the Reverend Joseph Emerson, a Congregational minister, who to the thoroughness of his teaching brought a high opinion of the intellectual powers of women. As Mary wrote, he "treated ladies and gentlemen essentially in the same manner." At Byfield Mary met Zilpah Grant, another pioneer teacher, with whom she would be associated through much of the decade 1824-34.

As she gained experience, Mary Lyon desired to establish a permanent, properly financed institution for the higher education of women. In Joseph Emerson's words, she looked to a time when "female institutions, very greatly superior to the present, will not only exist but be considered as important, as are now our colleges for the education of our sons." In 1834, Mary gave up her teaching and for the next three years devoted her entire time to raising money and laying plans for the new institution—writing all the appeals, raising the funds, and assembling the committee that became the board of trustees. South Hadley, Massachusetts, which

pledged \$8,000, was chosen as the location, the institution was named the Mount Holyoke Female Seminary, a five-story building was erected, eighty young ladies were admitted, and Miss Lyon and three young women began instruction on November 8, 1837.

The academic program covered three years and the curriculum was based on that of Amherst College. There was no preparatory department and all incoming students were required to take entrance examinations. The work of the regular teachers was supplemented by lecturers from Amherst and Williams. To lower costs and bring the seminary within the reach of families of moderate means, each student was assigned a specific household task. Education at Mount Holyoke was seen as a preparation for service among all classes and in foreign lands. A woman of deep Christian faith, Lyon was an ardent supporter of the American Board of Commissioners for Foreign Missions. She was to have only twelve years at Mount Holyoke, but when she died in 1849 she had made a permanent contribution to higher education.

This attractive, well-illustrated volume reintroduces Lyon to our own time. The impressive bibliography, extensive notes, and excellent index will be helpful to other researchers in the field.

E. WILSON LYON
Pomona College

JONATHAN LURIE. *The Chicago Board of Trade, 1859-1905: The Dynamics of Self-Regulation*. Urbana: University of Illinois Press. 1979. Pp. xiv, 234. \$12.50.

The Chicago Board of Trade, founded in 1848 as an informal club for local merchants, acquired a formal charter from the Illinois legislature in 1859. The charter invited the board to administer its own internal affairs as a commodities exchange, to appoint grain inspectors and weighers, and to regard its arbitration awards in members' disputes as the equivalent of Circuit Court judgments.

Jonathan Lurie draws upon a close reading of board records and papers, contemporary newspapers and periodicals, and a lengthy list of court cases to describe how the board subsequently developed and administered its delegated legal authority. The result is a book that illuminates the politics of economic self-regulation in the late nineteenth century and contributes usefully to the historical literature on the interaction of law, politics, and economic interest groups in the generation of American public policy.

In the two decades after its charter grant, the board hammered out its basic trading practices and forged an administrative framework. In aiming to provide an efficient and predictable market mecha-

nism for its members, who numbered over 1,600 in 1873, the board's directorate had to balance the need for collective discipline against its own limited leverage and the epoch's enormous speculative enthusiasms. As might be expected, internal self-discipline, "slow and often contradictory," lagged well behind speculative compulsions (p. 36). Rules against privileged trading and market corners were passed but not enforced.

Between 1874 and 1894 pressure for self-discipline mounted. Agrarian discontent stalked the land, and Lurie describes the congressional battle that agrarians waged for regulated futures trading. The agrarians lost, but the board confronted an even greater threat in economic competition from the so-called bucket shops. These small trading operations catered to commodity gamblers who objected to the board's \$10,000 initiation fee and rejected its rules on trade sizes, delivery, commission rates, and so on.

The board's struggle to close down the bucket shops was a slow, intermittent affair. Many of its own members retained bucket-shop connections, and the operations of the two institutions were nearly indistinguishable. In the long run, however, the board prevailed, and Lurie examines the court cases that marked the road to victory. The board eventually gained the right to deny all its market quotations to bucket-shop traders and to have all its own members barred from bucket-shop connections. By the turn of the century the board's functions had clearly won full legitimacy as quasi-public responsibilities.

The historical experience, moreover, held important implications for federal intervention when it did come. Lurie does not go into detail on the politics of the Grain Futures Act of 1921, the basis for all subsequent federal commodities legislation, except to note that it essentially wrote private practices into federal law. But in suggesting how the preceding history of self-regulation in a private economic institution such as the Chicago Board of Trade fundamentally shaped subsequent efforts at public regulation, Lurie has identified a key pattern in the evolution of American public policy.

ROBERT D. CUFF
York University

BRUCE BRINGHURST. *Antitrust and the Oil Monopoly: The Standard Oil Cases, 1890-1911*. (Contributions in Legal Studies, number 8.) Westport, Conn.: Greenwood Press. 1979. Pp. x, 296. \$22.95.

This book indicts American antitrust policy and its implementation on the basis of precedent-setting cases against Standard Oil, the archetype of American big business. The author concludes, "Antitrust

has never worked, and it has been used repeatedly to obscure the inability of the federal government to protect the public from the excesses of big business" (p. 222). In arriving at this verdict, Bruce Bringhurst examines the motives and impact of antitrust suits against Standard Oil at state and federal levels, culminating in the 1911 United States Supreme Court decision that ordered dissolution of the combination.

Four chapters are devoted to essentially futile efforts to invoke antitrust laws at the state level against Standard Oil. An early victory against Ohio Standard was neatly negated by reorganization of the Standard Oil Trust. When Ohio Attorney General Frank Monnett brought suit, he encountered resistance in obtaining company records, was subjected to adverse publicity at the hands of a Standard-subsidized press, and was politically pressured by U.S. Senator Joseph Foraker, who was in Standard's pocket. Monnett hurt his own credibility by alleging bribery and record-burning by Standard Oil. The case dragged on after he left office, and his successors showed little interest in taking aggressive action against the trust. One even claimed that for three years he was unable to serve a subpoena on a Standard company in Ohio!

The story was similar in Texas, Missouri, and Kansas. In Texas, for example, the appearance of successful action to oust a Standard subsidiary received widespread publicity, but the state continued to have Standard companies under new names. Kansas and Missouri wanted the investment and jobs that went with industrialization, and Standard Oil provided both. This fact, combined with the economic conservatism of judges, effective lobbying, and judicious use of political influence and wealth by Standard, created public relations triumphs and political rewards for the antitrust prosecutors but did little to change the competitive situation. When public concern reached national levels, the states seized on the claim that the initiative was being taken by the federal government as an excuse for inaction on their part.

Bringhurst shows that President Theodore Roosevelt used antitrust much as politicians had at the state level. His sense of timing, for example, led him to ignore citizen complaints against the oil combination but then to stir up and use public hostility toward Standard Oil to gain desired legislation. His efforts to amend the antitrust law to recognize "reasonable restraints" of trade failed, but that object was achieved in the judicial "rule of reason," advanced by Chief Justice White in the 1911 Standard Oil decision. One result was creation of a favorable legal climate for the merger movement of the 1920s.

The author makes his case, though it is somewhat weakened by his reluctance to concede that there

may be some virtues in bigness and that antitrust policy may have had some beneficial long-run effects on the structure and behavior of the oil industry. His attempt to cover developments down to the recent past in a single chapter is not in keeping with the in-depth treatment of the 1890-1911 period. Nevertheless, this book documents convincingly the key role of antitrust in soothing the American public about the maintenance of competition among small business units while the economy was being transformed by the rise of large-scale enterprise, with Standard Oil among its leaders.

ARTHUR M. JOHNSON
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MARK WYMAN. *Hard Rock Epic: Western Miners and the Industrial Revolution, 1860-1910*. Berkeley and Los Angeles: University of California Press. 1979. Pp. x, 331. \$15.95.

In recent years, a number of first-rate studies concerning the metal or hardrock miners of the American West have appeared. Among the best of these works are Richard E. Lingenfelter's *Hardrock Miners* (1974) and John Rowe's *Hardrock Men* (1974), each of which focused on particular aspects of the miners' life and work. Mark Wyman's book is a similar study, one which, although covering much familiar ground, nevertheless offers significant insights into the miners' responses to the problems resulting from the introduction of an ever-changing technology into a frontier environment.

Wyman demonstrates that the new technology was a mixed blessing. Technological innovations that were expected to make the miners more productive yet alleviate some of the hardships of the workplace often generated a host of new and complex problems for the miners—toxic gases, silicosis, electrocution, delayed and premature explosions, and so on. Industrializing the mines was achieved at great human cost as the miners learned to use machines, dynamite, and electricity deep in the earth. The new technology simultaneously revolutionized the organization of the work force, drastically altered the methods of financing mining operations, frequently imposed an absentee ownership, tied Western mining to the national and world economy, and gradually altered the institutional framework within which it was applied. These changes directly and often adversely affected the miners.

According to Wyman, the hardrock miners reacted to the new conditions in a variety of ways; for example, leaving the "country," seizing the mines, lobbying the state legislatures, resorting to violence, or forming unions. Some of these responses were obviously more significant than others. In analyzing

the responses, the author has modified some long-held views concerning the hardrock miners. For example, he rejects older interpretations of the causes of Western working-class radicalism and, instead, concludes that its origins "stemmed most immediately from a mounting sense of desperation" produced among hardrock miners by the new industrial climate (p. 227). He also concludes that the union movement among the miners was a "conservative, protective movement" (p. 173), primarily designed to maintain the high wages that had characterized Western metal mining from its beginning. Only occasionally did other issues such as union recognition assume equal importance, although Wyman concedes that the eight-hour day issue often ranked with wages as a cause of labor-management disputes. Wyman de-emphasizes the violence traditionally associated with unions of hardrock miners (for example, the Western Federation of Miners) and stresses the more typical peaceful resolution of labor-management problems in Western metal mining.

Generally, *Hard Rock Epic* is both a descriptive and analytical study of the impact of technology on the life of metalliferous miners of the West. It is thoroughly researched, drawing heavily upon primary sources and the most relevant recent scholarship concerning the hardrock men. The study is judicious and balanced with a somewhat revisionist slant on such issues as the origin and extent of the miners' radicalism. It fits well into the growing body of scholarship on Western metal mining. Historians of labor and the American West will find this volume instructive and a definite contribution to their fields of study.

GEORGE G. SUGGS, JR.
Southeast Missouri State University

JON C. TEAFORD. *City and Suburb: The Political Fragmentation of Metropolitan America, 1850-1970*. (Johns Hopkins Studies in Urban Affairs.) Baltimore: Johns Hopkins University Press. 1979. Pp. vii, 231. \$15.00.

The American suburban experience, although nearly as old as the country itself, has largely remained the preserve of political scientists and sociologists. A few historians have dared to venture forth, but the literature has been sparse. Jon C. Teaford's fine monograph thus fills one of the many gaps.

As his subtitle suggests, Teaford is interested in the position of suburban government in metropolitan America. The book is about political arrangements; it does not concern itself deeply with the great economic and social changes that transformed the way and where Americans lived. The existence

of large and growing populations on the periphery of major cities is taken for granted; what fascinates Teaford is the legal and political status of the thousands of communities these people established.

To understand why autonomous units of government proliferated after 1850, one must look first at the American commitment to local self-determination. Convinced that local affairs should be handled by local authorities, state legislatures enacted permissive incorporation laws. Unlike the approach in other countries (Teaford has a useful chapter describing the British approach) the responsibility for local government organization was left almost entirely to the local populace. So empowered, groups used the government to secure their own particularistic ends (for example, low taxes, prohibition, maintenance of social distinctions).

But by about 1880 fragmentation was being rivaled by consolidation. Benefiting from economies of scale, large cities were able to provide a wider range of services than their suburbs. Faced with a choice between autonomy and material comfort, many suburbs opted for the latter and joined the central city. Like the earlier incorporations, annexations occurred without regard to metropolitan administration; again, local self-determination reigned supreme.

After 1910 the consolidation movement all but disappeared. Improved technology and a stronger bond market allowed the larger, older suburbs to provide more services than in the past. Able to offer such amenities as transit, water, and sewerage and with particularism gaining strength as a consequence of Progressivism, the suburbs' incentives to merge with the central city evaporated.

Teaford's analysis of metropolitan fragmentation, consolidation, and suburban ascendancy through 1940 amplifies the findings of others. His research and case studies illuminate the many variations yet basic similarities in the experiences of American communities over the century. The second half of the book details the futile efforts in the 1920s and 1930s to reconcile the American traditions of dispersed power and centralized control through a federal form of metropolitan rule. The author clearly favors the idea, but he gives a dispassionate explanation of its failure. To the discomfort of those who claim the existence of a "power elite," Teaford demonstrates that the struggle for a federative metropolis pitted the business-, academic-, media-, and political-establishment against the farmers, factory workers, and the small-town politicians, and the latter prevailed. Pluralism was still a force.

A few caveats should be noted. Teaford accepts at face value the rationality and efficiency arguments of those favoring a federative metropolis; but power would thereby be redistributed and it should be made clearer who the winners and losers would

be. Although documented to 1970, the book covers the period since 1940 only sketchily. There is, for example, no discussion of the federal government's attempts at metropolitan reorganization, which, although not notably successful, does strike at local autonomy. Such openings to further research, however, characterize path-setting books, and this is one.

MARK I. GELFAND
Boston College

T. LINDSAY BAKER. *The First Polish Americans: Silesian Settlements in Texas*. College Station: Texas A&M University Press. 1979. Pp. xiv, 268. \$14.95.

Awareness of ethnic diversity in Texas increases constantly, thanks to efforts by such scholars as T. Lindsay Baker and the continuing work of the Texan Institute of Culture in San Antonio. Texans knew all along they counted Hill Country Germans and Southwestern Mexican-Americans in the midst of an Anglo-Celtic majority, and since the 1940s they also made room for the modern urban Yankee as a sub-ethnic species. Baker and other scholars now make them aware of a considerable contribution to Texan and United States economic and social growth in the Southwest.

Baker's group, the Poles, claim the first Polish colony in the United States as well as the first Polish Catholic Church and the first Polish schools among their contributions. These were planted in the hill country some sixty miles from San Antonio at Panna Maria (Virgin Mary) and Bandera. The migration came from Upper Silesia, then the southeastern end of Prussia, in the mid-1850s. It started because a Polish youth, Leopold Moczygamba, entered the priesthood and developed a missionary zeal that found opportunity in personnel shortages in Bishop Jean-Maria Odin's area in Texas. Odin recruited Father Bonaventure Keller, Keller recommended Moczygamba, and together they came to Texas in 1852.

Moczygamba's letters to his family, also widely read by relatives and neighbors, described abundant and inexpensive land and weather that remained pleasant and mild. They do not seem exaggerated or even read like recruiting letters—at least in the beginning. But the letters arrived among Poles dominated by German political authority, where poverty from high food prices, natural disasters, blights, droughts, epidemics of typhus and cholera, and increasing criminal activities made life unpleasant and dangerous. In 1854 major rains collapsed levees and dams, thereby flooding out two-thirds of the Silesian crops. Moczygamba's letters mentioned none of these problems and promised, perhaps by implication, much happiness. So they came.

In 1854, one hundred and fifty Poles, quickly termed "Polanders" by the Texans, crossed the ocean to establish their kind's first colony in the United States (individual Poles had of course migrated before, especially after the insurrection in 1830). The next year seven hundred friends and neighbors joined them, and five hundred more came in 1856. Then their numbers lessened as a result of things getting worse in America and better in Poland.

The immigrants represented the peasant class exclusively. They owned land in Poland and intended to own land in Texas; they paid taxes, worked hard, and eventually prospered. Their journey began with the liquidation of real and personal property, continued with a train ride to a seaport such as Bremen, dragged on with a monotonous, two-month ship passage to Galveston or Indianola, and concluded with an inland ride on a Mexican-driven donkey cart to Panna Maria or Bandera or one of the later-established settlements. Then came reality. No prepared site, no shelter, rattlesnakes, hostile Indians, occasionally inhospitable Anglos, and grasshoppers, among other hardships, wanted immediate and often continuing attention. Continental agents such as Julius Henrick Schüller helped them make the passage, and Father Moczygemba and earlier arrivals welcomed them, but hard times usually marked their migration. In fact, within a few years hard feelings against Moczygemba over their problems caused him to move to Detroit, Michigan.

Father Anthony Rossadowski and Father Julian Przysiecki carried on the Polish Catholic ministries until they also left. Following the American Civil War the Congregation of the Resurrection resumed Catholic service to the Poles. Few Poles served under Texas arms in the Confederacy, and almost none did unless conscripted. If captured, they accepted service in the Union army. Such "disloyalty" earned the Texas Poles more ill-will from the Anglos and resulted in their first real ethnic persecution in the troubled years of Reconstruction. But their problems brought forth solutions: they fought back for the first time entered politics and became more a part of the mainstream, while maintaining as much ethnic solidarity as possible at home.

They remain, as Baker observes, an island of Poland in a sea of Anglo-German-Mexican Texans. Baker handles his subject well. He writes pleasantly, and his documentation testifies to his familiarity with Polish and the Poles. His regard for that nation's people and their American descendants is as touching as it is obvious. I must wonder, though, how he got the Polish off-shoot colony of Franklin east of St. Louis on the Missouri River, why he frequently used the term southerner in reference to persecutors, or why he did not relate the Texas "Po-

landers" to the great migrations that peopled northern areas. But as a book on the Texas Poles, it is excellent.

ARCHIE P. MCDONALD

Stephen F. Austin State University

PATRICK F. PALERMO. *Lincoln Steffens*. (Twayne's United States Authors Series, number 320). Boston: Twayne. 1978. Pp. 148. \$9.95.

Twice during his lifetime, Lincoln Steffens was a cult figure, once as a muckraker during the 1900s and again as an autobiographical pilgrim on the left in the 1930s. Gone for more than forty years, he still continues to fascinate. Patrick F. Palermo offers "a critical-analytical study . . . not a biography" (p. 9). Based on published writings and letters and informed by the work of other historians, Palermo provides a searching account of Steffens's intellectual pilgrimage from liberal reform to Russian revolution. Like other critics, Palermo properly approaches *The Autobiography of Lincoln Steffens* (1931) with caution, but he grasps its central reality as "the tale of a man measuring himself as a moral agent in the world" (p. 11).

Steffens was an unpromising fiction writer and a not much better political philosopher. By all accounts, including his own, he was a great journalist. Bosses, businessmen, presidents, cops, and criminals opened up to him. He was a superb interviewer and storyteller, working with the Christian metaphor and plot through which the middle classes comprehended the American scene. With investigative journalism's combination of documentation and indignation, he offered both detective story and titanic battle between boodlers and reformers. The purpose was to shame the citizenry into democratic moral action. The villain, however, was not the boodler, boss, or even big businessman, but rather privilege, the edenic temptation-apple; and the heroes were the strong men, often reformer-bosses, who fought for the people. Progressivism, Palermo explains, was spiritual, not economic, aiming at the heart, not the system; Christ, not Marx. But it failed. The decline of muckraking and progressivism, the failure of his "Boston Plan" and of his attempts to mediate both the trial of the Los Angeles Times dynamiters and the Russian Revolution, plus the Versailles treaty, "broke Steffens' liberal faith" (p. 108). Still seeking but unable to still believe in progressivism, Steffens adopted the radicalism of the Russian Revolution, Lenin rather than Theodore Roosevelt, Tom Johnson, or Robert La Follette, as the leader, and historical determinism rather than reform and Christian transformation.

Palermo does not hesitate to theorize. Perhaps his most clarifying perception is that Steffens sought to

have it both ways. In seeing himself as a "broken liberal" with democracy rejected, compassion gone, and willing to accept the "necessary" harshness of historical process, Steffens particularly seemed to enjoy attacking his former liberal allies. Yet, he never formally entered the Communist Party. Perhaps his life-long liberalism, which "disqualified him from judging the revolution" (p. 112), also kept him from joining it. Like Moses, he was not to cross over into the promised land. Justin Kaplan's easily flowing biography (*Lincoln Steffens: A Biography* [1974]) is illuminating about Steffens's life, and Patrick Palermo's briefer account offers excellent guidance for his politics. Even so, Lincoln Steffens remains more interesting than all of us who write about him; that is why we still do.

DAVID CHALMERS
University of Florida

LOUIS FILLER. *Progressivism and Muckraking*. (Bibliographic Guides for Contemporary Collections.) New York: R. R. Bowker. 1976. Pp. xiv, 200.

Louis Filler has written a thoughtful and readable interpretative bibliography of Progressivism and muckraking, following his theme through the nineteenth and twentieth centuries and basing his essay on intensive and extensive reading in literary and historical materials. He evaluates the pertinent literature that presaged the Progressive impulse and muckraking, thoroughly examines the important works of and about the Progressive era, traces its continuing importance through the succeeding decades, and finally touches upon its meaning for the present. If current writing in this vein were to reach a large enough audience to be effective, he suggests, it would have to drop its clichés about businessmen and fulfill the emotional and literary needs of the public.

Two aspects of this slender volume limit its effectiveness, at least for this reviewer. While Filler recognizes that the Progressives' "major cause appeared to be monopoly, with its challenge to American traditions of freedom" (p. 72), the sentence only appears three-fifths of the way through the book, and the reaction to trusts is casually mentioned earlier in a discussion about labor and capital. Since the economic and political threat of private economic concentration was central to Progressive thought, the de-emphasis of this theme unbalances the volume. Without this focus we cannot see the change in Progressivism by the sixties that enabled the Kennedy-Johnson administrations—little concerned about oligopolies—to give us Telstar and social reform.

The second reservation is his treatment of socialism as "a radical wing to progressivism" (p. 59), a

common, but mistaken premise. While the spread of both movements derived from a growing concern with the overweening power of big business, and socialists were willing to support Progressive palliatives while Progressives borrowed from the immediate program of the socialists, they differed fundamentally on the ownership and operation of the means of production. While Progressives were aware of the importance of economic power in society, unlike the socialists they felt that corporate power could be restrained while ownership and decision making remained in private hands, a faith not borne out by history. As a result of his definition Filler illustrates a point about Progressivism exclusively with two socialist authors, and this could be misleading.

Nevertheless, the author has penned a fine work and he is entitled to his own structure and definitions.

FRED GREENBAUM
Queensborough Community College,
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ARTHUR S. LINK. *Woodrow Wilson: Revolution, War, and Peace*. Arlington Heights, Ill.: AHM Publishing. 1979. Pp. viii, 138. Cloth \$9.95, paper \$5.95.

In 1957 Arthur Link distilled his views on Woodrow Wilson as a world leader in a slim volume, *Wilson the Diplomatist*. For diplomatic historians the contribution was of immense value. Now Link has embodied his changing convictions in a markedly different book, *Woodrow Wilson: Revolution, War, and Peace*. In his earlier volume the author recognized weaknesses in Wilson's effort to avoid or ignore the realities that frustrated his principles. In his more recent effort he has eliminated such criticism and has produced, in large measure, the ultimate defense of Wilson's leadership. The list of achievements that conclude the first essay is long and impressive. While acknowledging that Wilson's acceptance of British maritime practice served the American interest in a British victory, the author, in his second and third essays, has introduced new and valuable material, especially a long note that Wilson dictated in the autumn of 1916, to substantiate Wilson's concern for a peace without victory. In accepting war in April 1917, Wilson hoped to terminate the war quickly and to establish his leadership in the building of a new postwar order. That Wilson failed to achieve an ideal peace at Versailles the author attributes less to Wilson's failures than to the special barriers he faced in the demands and personalities of Clemenceau, Lloyd George, and Orlando. Link does not attribute Wilson's loss of power to such factors as his refusal to negotiate with the Allies while they still required American aid or his deci-

sions to introduce the peace issue into the 1918 congressional elections, to lead the American delegation at Paris in person, and to ignore Republican Senators in his selection of the peace commission.

In this new volume the author upholds Wilson's decision to protect his treaty against reservations, for League membership on Wilson's terms alone, he agrees, would have assured a responsible exercise of American power. Wilson advocated a strong system of collective security, but he never answered the conflicting questions of interest, obligation, sovereignty, and power embodied in the problem of collective security. Still, the author offers no criticism of Wilson's vision of a new order of peace based on the League of Nations. He lauds the Paris settlement for establishing "the foundation of what could have been a viable and secure world order, if only the victors had maintained the will to build upon it" (p. 103). In the 1957 volume the author noted that Wilson's hopes for collective security ultimately took the form of regional alliances such as NATO. In the new volume he views the UN as the vindication of Wilson's vision. He concludes the book with a condemnation of those who ignored Wilson's plea for collective security and thus permitted another war to come. But did the failure of the democracies lay in not building up the League, as the author suggests, or in the refusal of the victors to maintain adequate national policies? Successful collective security in the 1930s would have reflected mutual interests and not membership in the League of Nations. League membership offered no substitute for effective national action; yet the specific interests of the United States worth pursuing and the power they required did not seem to concern Wilson at all.

NORMAN A. GRAEBNER
University of Virginia

DAVID BURNER *Herbert Hoover: A Public Life*. New York: Alfred A. Knopf. 1979. Pp. xii, 433. \$15.95.

Although over a decade of research and preparation went into this biography, it still leaves one with the impression that David Burner would have rather written a longer study about his subject and taken more time to do it. Whatever the reason, there is an unfinished quality to both the content and conceptual analysis that gives this book a sense of being hurried or incomplete. Indeed, Burner casts little light on the last third of Hoover's life between 1933 and 1964, although he does fill major gaps in our knowledge about the first third of it.

The opening chapters dealing with Hoover's formative years in Iowa, Oregon, and California, and his career as an international engineer are rich in detail. Hoover's far-flung activities before 1914 have never been so thoroughly documented. A later

chapter on his subsequent reform activities as president during the eight months prior to the Great Depression is probably the most original section. It clearly indicates that Hoover was well on his way to becoming the most progressive and successful Republican leader since Theodore Roosevelt. We learn about the "important and least-known actions" (p. 213) of the thirty-first president, including his stands on Indian rights, prison reform, land and energy conservation, the building of inland waterways, federal judiciary appointments, agricultural policy, and even his attempts at enhancing the civil liberties of blacks, women, and Jews.

One difficulty with this biography is that, aside from the first eight months of Hoover's ill-fated administration, Burner fails to offer particularly new or insightful material about the middle third of his career—the period between 1920 and 1933 when Hoover was secretary of commerce and a depression president. A more comprehensive conceptualization of his actions during these years can be found in Ellis Hawley's *The Great War and the Search for a Modern Order: A History of the American People and Their Institutions, 1917-1933* (1979). Moreover, the single chapter on foreign policy, along with a few isolated comments in other segments of the work, simply do not do justice to Hoover's systemic approach to foreign and domestic policy, especially during the years he headed the commerce department under Harding and Coolidge. This neglect of Hoover's influence on American diplomacy in the 1920s is particularly noticeable because Burner amply details the impact Hoover had on world affairs during the First World War. Crucial transitions in his thought on foreign and domestic issues between 1918 and 1921 such as the ideology of voluntarism, food diplomacy, the Russian Revolution, and international economics are given more attention in the collection of essays edited by Lawrence E. Gelfand, *Herbert Hoover: The Great War and Its Aftermath, 1914-1923* (1979).

Another analytical weakness of the biography arises when Burner compares Hoover and his brand of progressivism with the thought and actions of three other historical figures—Thorstein Veblen, Theodore Roosevelt, and Woodrow Wilson. He characterizes Hoover's foreign policy as in the tradition of Wilsonian internationalism (pp. 147, 184, 185, 313, 334, 336); his domestic policies as often like those of "his hero" Theodore Roosevelt (pp. 70, 150, 160, 178, 215, 297); and his engineering management style as most like that of Thorstein Veblen (pp. 63, 64, 67, 73, 75, 157, 210). Unfortunately, Burner does not discuss how Hoover managed to overcome the contradictions implicit in any blend of progressivism represented by these three men. It is also not made clear how their views could be squared with Hoover's seemingly contradictory

commitment to voluntarism, individualism, internationalism, nationalism, and "near" pacifism. Likewise, Burner does not elaborate upon a brief, but suggestive, comparison he finally makes between Hoover in 1933 and John Quincy Adams in 1829. In short, Hoover hovers unevenly over the landscape of his life in this biography, never completely coming into focus despite a few enticing glimpses.

One cannot help but conclude that Burner preferred to describe Hoover's early life and times. Otherwise, how do we deal with his explanation in the preface that the book does not detail his activities after 1933 primarily because of the "inaccessibility of hundreds of documents on the 1930s and after in the Herbert Hoover Presidential Library" (p. xii). In fact, the post-presidential papers represent one of the largest and least restricted of the major archival groups in the West Branch collection.

So we are left with an incomplete biography. While it does not contain the last word on Hoover, it does contribute significantly to Hooverian scholarship for the period before 1920. Arthur Schlesinger, jr.'s protestations notwithstanding (*The New York Review*, March 8, 1979), Burner documents beyond any reasonable doubt the generally progressive nature of Hoover's philosophy and actions.

JOAN HOFF WILSON
Arizona State University

RALPH D. GRAY. *Alloys and Automobiles: The Life of Elwood Haynes*. Indianapolis: Indiana Historical Society. 1979. Pp. xi, 243. \$9.00.

Falsely praised as the inventor of America's first successful automobile by many during his lifetime, Elwood Haynes (1857-1925) has been given only perfunctory mention by automotive historians. "Today he deserves a wider recognition, based not only upon a realistic assessment of his automotive work but also upon his noteworthy achievements in the field of metallurgy and for the cause of humanity," concludes Ralph D. Gray (p. 230). Handicapped by huge gaps in Haynes's correspondence files and the absence of business records for the companies with which Haynes was most intimately associated, Gray nevertheless succeeds both in establishing Haynes's true importance as an inventor and in providing some useful insights into neglected areas of the history of American technology.

Unique among the first generation of automotive engineer-entrepreneurs, Haynes was a college-educated chemist, primarily interested in metallurgy, who did graduate work at Johns Hopkins and taught school prior to playing a key role in the Indiana natural gas boom of the 1880s and 1890s as

Superintendent of the Indiana Natural Gas and Oil Company. Always more at home in his laboratory than in a business office or on a factory floor, Haynes characteristically turned over to others much of the administration of the various business enterprises with which he became associated through his inventions, and he hired Elmer Apperson to build the gasoline automobile he designed in 1893-94. Haynes-Apperson (1896-1905) became one of the first firms in the United States to manufacture motor vehicles, and Gray does an excellent job of detailing its history and that of its successor, the Haynes Automobile Company (1905-25), particularly in evaluating Haynes's contributions to automotive technology. Even more significant, however, were Haynes's contributions to metallurgy—most notably his 1912 patent on Stellite, a superior chrome alloy tool metal, and his patents on stainless steel. Gray demonstrates that these were significant technological accomplishments. They also led to the formation of both the Haynes Stellite Company (sold to Union Carbide in 1920 and now a division of Cabot Corporation) and the American Stainless Steel Company (1917-ca. 1933). A devout Presbyterian and staunch prohibitionist, Haynes remained active in church work and in the prohibition movement as well as a proponent of educational reform as a member of the Indiana State Board of Education from 1921 until his death in 1925. After World War I, he advocated American membership in the League of Nations and supported numerous anti-war organizations. And he was a leading early advocate of American adoption of the metric system.

Alloys and Automobiles is a well-researched, well-written, and informative book that deserves to be read by anyone interested in automotive history, the history of technology, or Indiana regional history.

JAMES J. FLINK
University of California,
Irvine

MARCIA GRAHAM SYNNOTT. *The Half-Opened Door: Discrimination and Admissions at Harvard, Yale, and Princeton, 1900-1970*. (Contributions in American History, number 80.) Westport, Conn: Greenwood Press. 1979. Pp. xxi, 310.

Shortly after the establishment of national quotas in the immigration restriction law of 1921, President A. Lawrence Lowell urged the reduction of the proportion of Jewish undergraduates at Harvard to about 15 percent. The Harvard Faculty of Arts and Sciences, informed that Jewish enrollment had tripled since 1900 (from 7 to 21 percent), first voted that admission procedures should take into account the proportion of racial and national groups and

then rescinded the action and awaited the report of a committee. While the committee deliberated, attacks on Lowell's idea mounted among Harvard undergraduates and alumni and in the press. A rumor that Harvard planned to limit admission of Irish brought Mayor Curley's denunciation and threats to remove the university's tax exemption. The committee's report rejected any racial or religious discrimination (upholding "traditional policy") and called for firmer bars against academically weak students. This policy was voted by the faculty, and the press praised Harvard for its vindication of democracy.

Lowell now called his admission plan crude, but he had not given up. A new proposal to limit the size of entering classes offered a less direct route to a Jewish quota. An Overseers-Corporation-Faculty committee headed by Henry James successfully recommended a plan to limit the size of each class to one thousand and give the admission committee broad discretionary powers in judging "character and fitness and the promise of the greatest usefulness" (p. 109). In the wake of this decision of 1926, the admission committee required photographs on application blanks, undertook more interviewing, and asked headmasters to identify applicants' religious preferences. A Yale administrator reported after a visit to his Harvard counterparts that they were "going to reduce their 25% Hebrew total to 15% or less by simply rejecting without detailed explanation" (p. 110). Lowell had achieved his aim silently through the university's bureaucracy. The new procedures held until World War II. As late as 1942, a memorandum referred to rejection of applicants because of "the 'quota'" (p. 112).

This episode, known only partially even to those most deeply concerned, is meticulously reconstructed by Marcia Graham Synnott. She treats Yale and Princeton, as well as Harvard, policies directed against blacks, Catholics, and foreign students as well as Jews, and discrimination against enrolled students and in faculty appointments. The book ends with an even-handed summary of the Bakke case, which, though valuable in itself, deflected the author from a final appraisal of the rise and fall of ethnic exclusiveness in the Big Three. Such a conclusion would have helped a book that has its own admission problem. What data should get in and what be excluded? Synnott has erred on the side of inclusiveness, sometimes overproving her points and blurring her interpretation. Breaking new ground, she presumably sought to be prepared for any challenge about accuracy from offended institutional representatives.

This volume parallels Harold S. Wechsler's *The Qualified Student: A History of Selective College Admission in America* (1977), which stresses nineteenth-century origins of admission policies, the case of Columbia,

and the effects of open admission in New York. Both works ably recapture past attitudes and procedures, matters that could easily drift away in the mists of institutional self-promotion and selective memory.

HUGH HAWKINS
Amherst College

RANDALL K. BURKETT. *Garveyism as a Religious Movement: The Institutionalization of a Black Civil Religion*. (ATLA Monograph Series, number 13.) Metuchen, N.J.: Scarecrow Press, for American Theological Library Association. 1978. Pp. xxvi, 216. \$11.00.

The civil rights revolution and black power movements of the 1960s spawned an outpouring of popular and scholarly works about the black experience in the United States. Not surprisingly, many of these have dealt with the previously neglected subject of Marcus Garvey and his Universal Negro Improvement Association. The Garvey Movement, which flourished in the early 1920s and then gradually declined until Garvey's untimely death in 1940, was the first genuine mass movement among American Negroes. Garvey's charismatic message of heightened black consciousness, race pride, self-help, and African redemption also won for him a considerable following in Africa and among the scattered black diaspora elsewhere in the New World. Most writers on Garveyism have commented on its religious aspects, noting especially the establishment of a new black denomination, the African Orthodox Church, by a prominent Garveyite, Archbishop George Alexander McGuire. At the same time, the conventional view has held that with a few notable exceptions like McGuire, Garvey was opposed by the vast majority of the black clergy, which saw the movement as a threat to its status and influence.

Now, in this important contribution to the scholarly literature on the Garvey movement, Randall K. Burkett has convincingly demonstrated that earlier writers, including this reviewer, were wrong in their treatment of Garvey's relations with the black clergy and in many of their assumptions about the religious elements of Garveyism. The Garvey movement in its heyday in fact attracted the interest and open support of far more black churchmen—clergy and laity alike—than we have hitherto known. As St. Clair Drake points out in his perceptive introduction to the book, he grew up in close association with black ministers in the 1920s when his clergyman father served for three years as the International Organizer of the U.N.I.A., but it was not until he read Burkett's study that he realized the extent of the involvement of the black clergy with the Garvey movement.

Anyone who has tried to piece together the scattered and fragmentary records of the Garvey movement can appreciate the diligence with which Burkett has reconstructed the involvement of numerous individual black clergymen of various denominations with the Universal Negro Improvement Association. Equally valuable is Burkett's analysis of the religious elements of Garvey's philosophy and his consistent strategy of avoiding conflict with the black clergy by endorsing no official church for the movement—not even McGuire's new African Orthodox Church, which resulted in a temporary falling out between the two men. The U.N.I.A. was an avowedly Christian organization, and its symbols, catechism, and rituals gave it a quasi-religious character. But Garvey shrewdly developed its religious aspects to produce what Burkett describes as a kind of black civil religion, ecumenical in its appeal and broad enough to encompass the believer and non-believer alike. Not everyone will accept Burkett's conclusion that Marcus Garvey was the foremost black theologian of the early twentieth century (Garvey himself would probably have denied it), but there will be general agreement that this carefully researched and insightful study is a major contribution to our understanding of the Garvey phenomenon.

E. DAVID CRONON
University of Wisconsin,
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ABBY ARTHUR JOHNSON and RONALD MABERRY JOHNSON.
Propaganda and Aesthetics: The Literary Politics of Afro-American Magazines in the Twentieth Century. Amherst: University of Massachusetts Press. 1979. Pp. 248. \$15.00.

Afro-American magazines, both current and past, are among the most useful artifacts in reconstructing not only the realities of life in the black community but also how blacks have perceived and defined their minority position in America. *Propaganda and Aesthetics*, while not a social history of Afro-American magazines, provides a clearly written introduction to the shifting currents of twentieth-century political and cultural nationalism as writers and critics debated whether art justified itself or should be for the purpose of propaganda and racial progress and demonstration.

Abby Arthur Johnson and Ronald Maberry Johnson focus on two types of periodicals, those giving priority to political and racial issues over cultural expression and genuine "little magazines" with a specific creative mission. The publications of the early twentieth century were primarily race periodicals, not specifically the vehicles for literature,

preoccupied as they were with outlining political alternatives to the accommodation of Booker T. Washington. Yet in printing short fiction and poetry, they openly acknowledged that art had its role in racial progress. This principle was more clearly confirmed in the 1920s as *Crisis*, *Opportunity*, and the *Messenger* did much to shepherd the Harlem Renaissance, even though they were "political" journals representing social movements. The little magazines of that period, had they survived and been published with greater regularity, would likely have developed in greater depth the debate over art versus propaganda that was only implicit in most of them. Yet the Renaissance dialogue over black esthetics reached beyond the pages of black publications, as, for example, the positions enunciated by Langston Hughes and George Schuyler in the *Nation*; unfortunately, the authors neglect to tie these discussions into their subject.

The decade of the 1930s marked a decline in the literary interests of black periodicals, although the occasional little magazine or college review permits the authors to chart left, right, and centrist variations of the art and politics theme. A more coherent pattern, labeled "the aesthetics of integration," emerged in the 1940s and 1950s, which focused on improved race relations and their portent for broader black participation in society as a whole. But by the height of the civil rights movement in 1963, this vision of a multiracial future began to be attacked in several new little magazines. The enemy—white society and its black political and cultural lackeys—was easy enough to identify and vilify, but the creation of a single esthetic vision that could define the role of black art in a revolutionary political context proved impossible to construct; editors and contributors to the various magazines "could not achieve a consensus on the meaning of separatism, nationalism, and revolution. . . . As a result, the political implications of art-for-people's sake, a slogan used as a partial definition of the black aesthetic, never became sufficiently clear" (p. 198).

As the Johnsons make clear, there never was, nor likely will be, a single black esthetic. One can expect that the debates of past generations will be repeated in the future. What one finds lacking in this volume, however, is a deeper probing. What did the black periodicals have to say about the specific roles of poetry, fiction, drama, and criticism in achieving either a revolutionary separatist or an integrated future? They slight, for example, Theophilus Lewis's extensive discussion in the *Messenger* of the role of the popular theater in race liberation. One would also like some hints as to the importance, if any, of the magazines surveyed and their literary and critical contents to the lives of the masses, as distinct from the Talented Tenth. In short, the implications

of this study go far beyond the pages and readership of the periodicals studied in this volume.

THEODORE KORNWEIBEL, JR.
San Diego State University

JOHN W. JEFFRIES. *Testing the Roosevelt Coalition: Connecticut Society and Politics in the Era of World War II.* (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1979. Pp. xiv, 312. \$16.50.

By now it is truism that the political revolution encompassed in the Roosevelt coalition established the Democratic party by 1936 as the dominant party in American political life. Forged through those New Deal policies and programs affecting primarily urban areas, it brought various ethnic groups, Catholics, Jews, blacks, labor, and the poor into a political coalition neatly balancing ethnic and class divisions. John W. Jeffries uses Connecticut as a case study to examine these developments because, as he argues convincingly, its society and politics revealed much about the politics and society of similar heavily urban and industrial states, let alone the United States in general. By focusing on elections from 1936 through 1946 and utilizing sophisticated analytical techniques, a wide range of primary sources, and a clear, crisp narrative style, Jeffries persuasively demonstrates how during this decade the Roosevelt coalition made the party a powerful factor in Connecticut political life.

To be sure, the Democrats carried Connecticut only in those elections in which FDR headed the ticket, losing the state in every non-presidential election. But in these elections the parameters of the coalition were not destroyed, although top Connecticut Republicans, particularly Raymond Baldwin, made inroads on it by sponsoring legislation, instituting programs, and championing ethnically balanced tickets. In addition, the coalition was challenged by foreign policy issues such as the role of Italy in World War II, the fate of Poland, and the beginnings of the Cold War. But, despite these challenges, ample evidence is presented indicating that Connecticut elections were predicated on domestic issues concerning the economy and the role of government in assuring peace and prosperity. Though issues changed and new challenges arose, Jeffries abundantly records the continuity that prevailed in the structure of Connecticut society and hence in its politics. And his analysis nicely relates Connecticut to the national political scene, developing the interaction between national issues and state politics. His conclusion, an obvious and unexceptional one given his thorough and exceptional analysis, is that while the Roosevelt coalition did not make Connecticut a solid Democratic state, one

that party leaders could take for granted, it nevertheless forged a powerful two-party system, one in which elections would be sharply contested. For the Democrats to be successful they would have to hold the coalition intact and try to broaden the appeal to voters in Connecticut towns and suburbs, while the Republicans would have to take a progressive stance and make inroads on the urban bastions comprising the core of the coalition's strength.

His conclusion is also obvious and unexceptional because it was earlier presented in a penetrating study, *New England State Politics* (1959) by Duane Lockard, a student of V. O. Key. While Lockard's two lengthy chapters on Connecticut in many ways parallel and complement Jeffries's fine study, they are nowhere cited in his lengthy footnotes (twenty-two pages) or in his otherwise comprehensive bibliographical essay.

RICHARD LOWITT
Iowa State University

NORMAN W. SPELLMAN. *Growing A Soul: The Story of A. Frank Smith.* Dallas, Tex.: SMU Press. 1979. Pp. xiii, 513. \$15.00.

This biography narrates the life of a Texas clergyman who attained the rank of bishop in the Methodist Episcopal Church, South, in 1930, who served as first president of the Council of Bishops of the Methodist Church following the merger of his denomination with its northern counterpart, and who was particularly identified with several church-related enterprises, including Southern Methodist University, Southwestern University (Georgetown, Texas), and Houston Methodist Hospital. As much a curiosity as the book's title is the fact that another Angie Smith—William Angie, Jr., brother of the subject, Angie Frank—served concurrently as a Methodist bishop beginning in 1944.

Though the volume is based on extensive research and its subject was a figure of some renown, it will neither appeal to a wide readership nor win high marks as a scholarly contribution. It is too much a recitation of worthy goals attained, particularly in the matters of membership accessions, enlarged physical plants, fund raising, and the like. *Growing A Soul* can perhaps be of greatest use to students of the Methodist bureaucracy. Clearly, Smith was a dedicated shepherd, and, clearly, he had winning manners, a pleasing appearance, organizational ability, and a gift for public address. "You look like a bishop, you walk and talk like a bishop, and above all you preach like a bishop!" an admiring fellow prelate once exclaimed (p. 342).

On the other hand, Smith's rise to eminence also seems considerably attributable to a calculating eye for enlarged opportunities, to a compatibility with

affluent, establishment-oriented laymen, to ties with powerful senior churchmen, and to a knack for shunning controversy. Amidst a furor over the Ku Klux Klan during his pastorate at Houston's First Methodist Church, in the 1920s, Smith determined that "this issue was not to enter our board meetings" or cripple the church program overall (p. 135). With both a Klan organizer and a judge who led the attack on the Klan conspicuously involved in congregational affairs, Smith tactfully sermonized (as he later explained) on "the power of God to save, Sunday after Sunday" (p. 137). Appropriately, when Bishop James M. Cannon, Jr., a sometimes reform activist and political controversialist, inquired about hospitality in Houston during the national nominating convention of the Democratic party, in 1928, he learned that accommodations were unavailable at the Smith residence. A. Frank Smith stood out among his peers as one who "wasn't tagged as being pro this or pro that" (p. 229).

A dust-jacket blurb accurately advises that the book will help illuminate "the history of Texas Methodism in particular and American Methodism in general through half a century." It seems fair to say that Smith's own estimable contributions lay principally in the spheres of spiritual enrichment, administrative expertise, tact, and the quieting of contention.

KENNETH K. BAILEY
University of Texas,
El Paso

DORIS B. McLAUGHLIN and ANITA L. W. SCHOOMAKER. *The Landrum-Griffin Act and Union Democracy*. Ann Arbor: University of Michigan Press. 1979. Pp. vi, 288. \$22.50.

In 1959 Congress enacted the Labor Management Reporting and Disclosure Act (LMRDA), popularly known as the Landrum-Griffin Act after its authors. This law came as a result of strong public and congressional support, similar to that which twelve years earlier had prompted the Taft-Hartley Act. In the later instance, Senator John McClellan's investigations of abuses in the Teamsters Union had demonstrated a need to protect individual members from their own officials.

Depending mainly on questionnaires and interviews, this study seeks to determine (1) if the law improved internal union democracy; (2) the effect of court decisions in defining statutory language; and (3) the role of Department of Labor (DOL) officials in protecting individual rights. The book's appendixes include a reprint of the law and the procedures of the investigators as well as tabulations of the results of the questionnaires used. The au-

thors conclude that the overall effects of the law have been beneficial in furthering union democracy, and they recommend a number of changes for DOL officials in administering the policy. The study is concerned primarily with determining the opinions of those involved in the policy rather than with a history of how the law has operated. A detailed account of how this policy has affected the procedures of the Teamsters, Mineworkers, and other unions would be welcome, but that would be asking the authors for a different book.

This study was begun under a contract with the DOL. Why the University of Michigan Press subsequently published it is not clear to this reviewer. It obviously would be of benefit to the Department of Labor, both in recommending changes in the law to Congress and in revising its *LMRDA Interpretive Manual*. In fact, DOL officials have already implemented some of the suggestions. The historian of recent American labor history, however, will find it to be tedious reading and will discover little of substantive value.

R. ALTON LEE
University of South Dakota

ALFRED E. ECKES, JR. *The United States and the Global Struggle for Minerals*. Austin: University of Texas Press. 1979. Pp. xi, 353. \$18.95.

In his preface to *The United States and the Global Struggle for Minerals*, Alfred E. Eckes, Jr., informs his readers that "for purposes of organization" and because they have been or will be covered elsewhere, agricultural commodities and energy materials have been excluded from his coverage. As a result, except for occasional references, Eckes neglects that most crucial of global minerals in the twentieth century, oil. Moreover, because of the unavailability of source materials, Eckes's coverage of the 1960s and 1970s is superficial (he treats the Kennedy-Johnson years in seven pages). Finally, although sensitive to the danger of giving his work too deterministic a tone, some of his specific interpretations, especially his emphasis on raw materials as an explanation for Germany's thrust into the Soviet Union during World War II, seems strained (one also wonders why Eckes deals with such matters in a book concerned primarily with United States foreign policy.)

Indeed, Eckes's own analysis suggests that throughout much of the twentieth century, United States leaders actually ignored minerals as a major factor in the formulation of foreign policy. For example, until World War I, the mineral resource issue in the United States was seen only as a domestic one. Although World War I revealed to some the political importance of minerals, at Paris after the war Woodrow Wilson was not interested in sub-

stantive economic or natural resource issues. Likewise, before World War II President Roosevelt showed little concern with the minerals question, even blocking a materials purchase program.

To be sure, other Americans in and out of government recognized the world's resource interdependence and pushed for an official minerals policy along internationalist lines. In the 1920s, for example, Herbert Hoover sought to break up the foreign cartels controlling many of the world's mineral resources and, although Eckes mentions it only in passing, the state department made the opening of the Middle East to American oil interests a high priority. Other examples can also be cited.

As Eckes himself indicates, however, it was not until after World War II that the quest for secure mineral supplies became a high foreign policy priority (with the notable exception of oil). Faced with increasing demand for minerals, having outgrown its own domestic resources, and threatened with Soviet cannibalization of American foreign resource supplies, the United States opted for an internationalist minerals policy (one that paralleled President Truman's internationalist outlook), including a massive stockpile procurement program. As Eckes comments, by embodying this point of view the famous Paley Report of 1952 marked "a significant shift in national materials policy" (p. 198).

In other words, Eckes's own evidence seems to undermine much of his case as to the importance of minerals to American foreign policy, at least for the period prior to the 1950s, a case that, incidentally, could have been bolstered substantially had he given more attention to international petroleum policy. At least as important to American foreign policy as the proponents of an international minerals policy, it seems, were the opponents and the disinterested in policymaking positions. Eckes does not neglect these groups, but he fails to measure fully their impact on policy formulation.

Eckes appears on stronger ground in his coverage of the last two decades, although, as already indicated, his treatment of the period is necessarily thin. In the 1960s, the resource scarcity thesis (as opposed to resource exhaustion) on which the Paley Report rested was challenged by successive administrations, which were concerned about mineral surpluses and which sold off a large part of the nation's stockpiled materials. A combination of factors in the 1970s, however, including a growing world demand for raw materials, a decline in mining investment abroad, a consequent rise in prices, and continued world competition with the Soviet Union has led to a new scramble for mineral supplies and a renewed emphasis on minerals acquisition as a foreign policy priority. "The U. S. was too dependent on other nations for the commodities needed

to sustain an advanced industrial society," Eckes concludes, "for policymakers to ignore external events that might jeopardize economic security" (p. 255).

Although one can quarrel with some of its interpretation and omissions, *The United States and the Global Struggle for Minerals* does reveal that the resource concerns of the 1970s are not unique to this period.

BURTON KAUFMAN
Kansas State University

PHYLLIS R. PARKER. *Brazil and the Quiet Intervention, 1964*. (Texas Pan American Series.) Austin: University of Texas Press. 1979. Pp. xiv, 147. \$9.95.

In 1964, the Johnson administration gave moral support to, and prepared material assistance for, a right-wing conspiracy to overthrow the constitutional president of Brazil, João Goulart. Both the Brazilian conspirators and their American allies saw the hand of international communism in Goulart's efforts to mobilize urban workers and landless peasants. Afterwards they joined in chorus chanting that a coup d'état of the "democratic forces" against the legal president had been necessary to save Brazilian democracy. It is now clear that the only foreign hand violating Brazilian sovereignty was the American one. Moreover, the road to democratic salvation took a fifteen-year detour into an often nightmarish military dictatorship. In many ways the 1964 coup was a testing ground for methods employed against the Salvador Allende administration in Chile in 1973.

Anyone wishing to grasp the full extent of the disaster that scarred American diplomacy in the 1960s should contemplate the readiness of American officials to meddle in the internal affairs of its largest neighbor to the south. Phyllis R. Parker has used recently declassified American materials and interviews with Ambassador Lincoln Gordon and his military attaché, General Vernon Walters, and then Assistant Secretary of State Thomas Mann to reconstruct the steps that led to the creation of Operation Brother Sam, a stand-by supply effort, and to recognition of an illegally established government. Her analysis is thin and limited to presenting the American conspirators' views of the situation. Since the details of Ambassador Gordon's story apparently have changed over the years and seem to clash with the documentary evidence, it is pointless to use his testimony without corroboration, as Parker repeatedly does here.

The declassified material in the Kennedy and Johnson libraries is fascinating and might be worth publishing as edited, annotated documents, but their use did not justify publication of this volume.

Not only has Parker not utilized the extensive bibliography that has appeared in Brazil since 1964, but also she has ignored Jan K. Black's *United States Penetration of Brazil* (1977), which was available in dissertation form in 1975 and which covers the same ground more completely. By not insisting that she do a thorough job, the University of Texas Press did Parker and itself a disservice.

Its shallowness aside, the book has an important message that the American public needs to hear repeatedly. "There seems to have been," Parker concludes, "no conflict in the minds of U.S. officials over someone's believing in the democratic process and at the same time plotting to overthrow a constitutional democracy . . ." (p. 107). The question is, does someone who wants to overthrow democracy anywhere really believe in it? For those who were listening in 1964, the Brazilian intervention was anything but quiet. Sadly, subsequent interventions proved that too few had been listening.

FRANK D. MCCANN
University of New Hampshire

W. DAVID LEWIS and WESLEY PHILLIPS NEWTON. *Delta: The History of An Airline*. Athens: University of Georgia Press. 1979. Pp. xiii, 503. \$15.00.

This solid volume is one of the first scholarly histories of a major American airline. W. David Lewis and Wesley Phillips Newton were given full access to company records and they have written a detailed narrative account of the fifty-year history of Delta Air Lines.

The leading figure in the evolution of this pioneer southern airline was C. E. Woolman, a University of Illinois graduate who moved to Louisiana before World War I to work with the agricultural extension service. By 1925 the extension agent became involved in a crop-dusting venture in the delta country with headquarters at Monroe, Louisiana. The small business grew, and Woolman plus a few associates formed Delta Air Service, which in 1929 started offering passenger flights in a single, second-hand, six-passenger Travel Air monoplane flying from Birmingham to Dallas. Air traffic was light in the early 1930s and often crop-dusting receipts were larger than passenger revenues. Some improvement came in 1934 when Delta was granted a federal airmail contract for a route from Fort Worth to Charleston. During the remaining years of the decade airmail receipts were considerably greater than passenger income. Prosperity came with World War II. During the war the load factor increased from 43 to 90 percent, and the fleet of DC-3s increased from five to ten.

In the early postwar years Delta sought to expand through merger and in 1952 combined with

Chicago & Southern, a smaller airline serving such mid-American cities as Chicago, Kansas City, Memphis, and New Orleans. By 1953 the Delta-C & S Air Lines was the sixth largest airline in the nation with 4,200 employees, gross revenues of \$32,000,000, and a net income, after taxes, of over \$4,000,000. Three years later in 1956 Delta finally won approval from the Civil Aeronautics Board to extend its service to Washington, Baltimore, Philadelphia, and New York in "Main Street America" (p. 250). The workhorse in all this expansion was Woolman, founder of Delta and chief executive officer until his death in 1966, who kept employee morale high with the growth of "family feeling" throughout the workforce, who had the final word on new aircraft selection, who directed the CAB campaigns for route extension, and who reluctantly in 1958 agreed to allow Delta to serve alcoholic beverages in flight.

In 1960 Delta retired the last of its DC-3s, the same year that the new DC-8 jet transports took over from the DC-7s. By the time that Delta was fully accustomed to jet service, it had received CAB approval, in 1961, for new western routes from Dallas to Los Angeles and San Francisco. During the decade of the sixties gross revenues and revenue passenger-miles both more than quadrupled, while net income increased more than twelve-fold. In 1972 a merger with Northeast Airlines provided Delta with service in New England, and in 1978 it inaugurated transatlantic service from Atlanta to London. The growth of traffic and increasing prosperity continued in the 1970s. In 1977 a reporter called Delta: "Not the country's biggest airline; it doesn't have the most flights or fly the most passengers. It just makes the most money" (p. 394).

While this is generally a sympathetic corporate history, the authors do not cover up the faults and mistakes of the airline—rather full details are given of several serious aircraft accidents. In addition to their use of company records, Lewis and Newton had numerous interviews with active and retired Delta personnel and support their story with complete documentation. Dozens of pictures, nine excellent maps, and useful appendixes supplement the text. This study should appeal to both the general reader and the student of American transportation history.

JOHN F. STOVER
Purdue University

J. HARVIE WILKINSON III. *From Brown to Bakke: The Supreme Court and School Integration, 1954-1978*. New York: Oxford University Press. 1979. Pp. viii, 368. \$17.95.

J. Harvie Wilkinson III is a young white Southerner, former lecturer at the University of Virginia

law school and editor of the Norfolk *Virginian-Pilot*. His story of the Supreme Court and school integration from *Brown v. Board of Education* to the aftermath of the Bakke case is heavily narrative and, to a considerable degree, personal. During this period he sees the South (by which he means the white South) "progressing" in the area of school desegregation through four successive stages: absolute defiance, token compliance, modest integration, and massive integration. Out of the latter, Wilkinson sees a fifth stage growing—that of resegregation, at least in certain localities in the South, and more recently the North—a development that he traces from Charlotte and Richmond to Denver and Boston and on to Detroit, Pasadena, and Dayton. This he sees as a function of busing, for him an almost unqualified evil, his sequence here moving from "black take-over" through white flight to a new segregation with black school systems in the central city and white systems (plus private white schools) in the suburbs. Woven through this chronicle is a brief background on race relations leading up to Brown; and assessment of the Brown decision; an evaluation of the "protective role" played by the Supreme Court in "siring" the civil rights movement, succoring it, defending it, and (and here Wilkinson is speculative) more recently moving toward deserting it. Further, as Wilkinson constructs his picture, there is a sincere effort made, as controversial aspects of these developments occur, to present the arguments made by spokesmen on both sides of each controversy, whether this includes public figures from Supreme Court justices to governors and public officials, school board members, local editors, opinion forums, and even, in some instances, reactions of the "victims" of integration policy themselves, who are, by and large, middle- and especially lower-class whites.

The results, in many ways, tell more about the author than the events he treats, but, in the process, tell a great deal about a powerful constituency of like mind currently in central policy-making positions. While admitting freely that pre-Brown race relations produced oppression, ignorance, and injustice and acknowledging that in the early years of the civil rights movement there was only one side for people of good will, Wilkinson sees a second valid side emerging by the early 1970s as coercion to achieve school integration took more overt forms. For him, courts should now be asking whether the social and academic benefits actually produced by integration justify the risks and disruptions necessary to achieve it. Thus, as with the man for whom he formerly clerked, Justice Lewis Powell, whose Bakke ruling he gives a cautious endorsement, Wilkinson feels law should be used in such a way as to ensure social stability. The application of constitutional principles, when they lead to community hos-

tility, chaos, and upheaval, should be tempered in hopes of reaching a resolution on the race question more equitable to those for whom forced integration is too disruptive and traumatic. In this regard, busing raises vital moral questions and challenges fundamental principles: "the right to be left alone, to control one's own destiny, to live by one's own lights, not those of an unelected district judges or theorists whose notions of equality have become constitutional law."

Intriguingly, Wilkinson sees himself and the white South generally as having benefited from the positive forces that Brown unleashed. However, he also sees some of those forces as far less than positive, with a clear lesson revealed: "School integration has taught us at home what Vietnam did abroad; how much eludes the American capacity to reshape." And, for Wilkinson, continued heavy-handed attempts to reshape may well undermine the kind of progress with which he is comfortable in the race relations area.

PAUL L. MURPHY
*University of Minnesota,
Minneapolis*

G. WILLIAM DOMHOFF. *The Powers That Be: Processes of Ruling-Class Domination in America*. New York: Random House. 1978. Pp. xv, 206. \$10.00.

For years, G. William Domhoff has been arguing that a small class of rich property owners controls American business and politics, acting as a ruling class in the strictest sense of the term. Where other students of social stratification have been content merely to demonstrate the existence of an upper class, Domhoff tries to show that this class actually makes the key decisions in all the major institutions of American society. In his new book, he argues that members of the upper class dominate the "policy network," the selection of political candidates, and the formation of ideology. He takes issue with those who emphasize the importance of special interests, like Grant McConnell, and with theorists of the "power elite," like C. Wright Mills, both of whom fail to see, according to Domhoff, that the power of special interests springs from a larger pattern of class rule.

When he insists that the property-owning class "is as alive and well as it has ever been," Domhoff also takes issue with theorists of the "managerial revolution," who see the old elite as having been displaced by a new oligarchy of administrators, managers, and bureaucrats. As always, Domhoff's work provides a useful corrective to the cruder versions of this theory, according to which the managers care more about growth than profits and use their power for the good of society as a whole. But

his analysis cannot do justice to the historical developments that have made direct class rule a thing of the past. One does not have to accept either the idea of a managerial revolution or the pluralist theory of "veto groups" and "countervailing power" to accept the general validity of David Riesman's observation that "explicit class leadership" died with McKinley. The general crisis of authority is the most obvious sign of its obsolescence: the exhaustion of ruling class ideologies, the inability of elites to provide a coherent justification of their own power, the retreat from confrontations over principles, the resort to psychological manipulation in place of ideological coercion.

When Domhoff tries to show that the ruling class retains a coherent and fully worked out world view, he describes an ideology that now finds support only on the extreme right. Laissez-faire liberalism, he claims, continues to enjoy a "near-monopoly of American political thought." In fact, the ideology of "individualism, free enterprise, competition, equality of opportunity and a minimum of reliance upon government" long ago gave way to a therapeutic, permissive style of leadership that dispenses with appeals to authority, sides sentimentally with the underdog and the outcast, and seeks to co-opt dissension through programs of affirmative action and "innovative" social change. That such programs leave the existing distribution of wealth untouched does not establish the existence of a ruling class in any important sense of the term; for as Domhoff himself concedes—though reluctantly, for the sake of argument—we cannot "infer power from the distribution of wealth, income, health, education, and other benefits."

The United States today presents the curious spectacle of a capitalist society in which the capitalist class plays an altogether negligible role. No theory has yet managed to capture the ironies and contradictions of such a situation, and Domhoff's ruling-class theory does not even come close.

CHRISTOPHER LASCH
University of Rochester

CANADA

RICHARD J. DIUBALDO. *Stefansson and the Canadian Arctic*. Montreal: McGill-Queen's University Press. 1978. Pp. xii, 274. \$18.95.

Richard J. Diubaldo's study of Vilhjalmur Stefansson's early career in the Canadian arctic raises important questions about Stefansson and about science and government generally. Diubaldo's purpose is not to trace Stefansson's exploits in lavish and stirring detail but, rather, to discover why "a man whose northern work and ideas about Canada's

northern destiny had fired the imagination of Canadians" became "unofficially ostracized from Canada" in the 1920s and is largely forgotten today. Given Stefansson's accomplishments—the accumulation of large amounts of invaluable anthropological detail, the mapping of much of the arctic, the discovery of major islands claimed for Canada, and the development of techniques for living in the Arctic—his current obscurity in a country ransacking its past for hero-adventurers does seem puzzling. Diubaldo's study shows why it should not be.

Stefansson promised so much. His background had the material of which legends are made. Born in Gimli, Manitoba, of Icelandic parents who lost two children to the hardships of pioneer prairie life, Stefansson had only twenty-seven months of primary education but still completed his education as a fellowship student in anthropology at Harvard. From the beginning, Stefansson had a sense of the advantage of his background and an arrogant awareness of how he might use it. Of Harvard he wrote, it "is the place for the poor man who knows he is made of the right stuff, who has found by experience that he has more inferiors than superiors in ability and courage." And in 1899 "Willy" Stephenson became Vilhjalmur Stefansson.

A Harvard scientist of romantic northern background was naturally lured to the Canadian arctic at the beginning of this century. For their part, Canadians were drawn to Stefansson because science seemed to offer the means of establishing their claims upon an arctic region that common sense told them was too harsh for human habitation and exploitation. In the first years of the new century, common sense had already lost much ground to the claims of science. The Canadian west, which had long resisted settlement and development, was booming because of the railway, the new strains of wheat, and so many other "scientific" innovations. If science had made the west habitable and even prosperous, why could it not do the same for the north? By 1908 Stefansson was writing in *Harper's* that "A reasonably healthy body is all the equipment a white man needs for a comfortable winter among the arctic Eskimos." The white man, of course, wanted to hear this, and he gave Stefansson support for the next expedition. Stefansson saw the advantage of publicity, and it became for him, Diubaldo writes, "the key, or at least a short cut, to success." Yet, when the key opened the door, the path led only to disillusionment and failure. Although publicity won Stefansson entry into the prime minister's office, it also created suspicion of his motives and, justly, his scientific integrity; when Stefansson's expeditions did not produce the results the public and the politicians had been led to expect, his detractors gained the upper hand. The central issue of arctic development and exploration was lost in the

controversy about Stefansson himself. What Stefansson the publicist promised, the scientist always knew was impossible to attain. Canadians began to look elsewhere as Stefansson's scientific colleagues dissected what was left of his arctic dream.

Diubaldo has written an important book based upon thorough research in manuscript sources. He has not always used his material as effectively as he might, and he does not portray Stefansson "in human terms" as the dust-jacket suggests he will. Perhaps only a novelist could. Diubaldo is not a novelist; this book does show that he is a good historian.

JOHN ENGLISH
University of Waterloo

ESCOTT REID. *Time of Fear and Hope: The Making of the North Atlantic Treaty, 1947-1949*. Toronto: McClelland and Stewart. 1977. Pp. 315. \$13.95.

This important contribution to the historiography of the North Atlantic Treaty Organization appears at a time when historians, as opposed to political scientists, have just begun to turn their attention to the subject. Escott Reid, a Canadian diplomat then in the early stages of a distinguished career, was second in command in the Department of External Affairs in Ottawa from April 1947 to March 1949. His chief was Lester Pearson, one of the principal architects of NATO. In this capacity Reid was an active participant in the diplomacy leading to the North Atlantic Treaty. Although he does not use the Achesonian term, "present at the creation," he implies that he was not only more intimately involved than Acheson had been but also a creator of the treaty. In the summer of 1947 he believed (p. 31) that his may have been the first public statement advocating a collective defense system for the Western powers.

His insider's knowledge, however, does not make this just a personal memoir. With careful scholarship he assembled his notes, analyzed the files of the Canadian foreign office, and exploited interviews with such leading American policy makers as John D. Hickerson and Theodore C. Achilles to reconstruct and evaluate the negotiations among the twelve nations of NATO. The result is a full treatment of the complicated negotiations that began with the Brussels Pact of March 1948 and proceeded through four stages ranging from secret negotiations at the Pentagon among the United States, Canadian, and British representatives in the spring to discussion with the Western powers in the summer, to a draft treaty in December, and lastly to working out a final text between January 10 and March 28, 1949. The critical issue was always the extent of America's pledge to frightened Europeans, and the breakthrough was the Vandenberg Resolu-

tion of June 1948. Much of the delay in the completion of negotiations derived from an expectation that no serious American commitment would be concluded until President Dewey was in the White House.

As he traces the course of negotiations, Reid enlivens his book with personal comments. We know his heroes—Hickerson, Achilles, Robert A. Lovett, Arthur H. Vandenberg, Gladwyn Jebb, and Lester Pearson. Although there are no enemies as such, George F. Kennan and Charles E. Bohlen of the United States and Hume Wrong, the Canadian ambassador, are criticized for insufficient recognition either of the imperatives of a multilateral commitment or of the importance of an Atlantic community. Acheson's ability and incisive style Reid balances against his "arrogance of expression," which irritated ambassadors (p. 150). Among the many virtues of the book is a vivid presentation of a Canadian point of view as its diplomats opposed an American unilateral commitment to Europe that would isolate Canada. Ultimately, the friends of the alliance triumphed, just as hope triumphed over fear in 1949.

There are some problems with the work. Although his writing is clear and occasionally stylish, the organization of the material frequently produces repetition. And, although he is justified in seeing himself as a founding father, even creators have lapses. Reid misplaces the date of the signing on one occasion (p. 95) and recalls a meeting with Dulles in the early 1960s (p. 245) on another. Elsewhere (p. 232), he admits that his failings as a diplomat include overzealousness and perfectionism. This should not be an excuse, however, for a reviewer to apply these traits to a criticism of a book that every scholar in the field should read with appreciation.

LAWRENCE S. KAPLAN
Kent State University

JOHN P. SCHLEGEL. *The Deceptive Ash: Bilingualism and Canadian Policy in Africa, 1957-1971*. Washington: University Press of America. 1978. Pp. xviii, 463. Paper \$14.50.

The Deceptive Ash is John P. Schlegel's doctoral dissertation at Oxford, apparently published here without significant alteration. From the standpoint of presentation and format, therefore, it demonstrates the limitations of theses as a genre and may suggest the advantage of indulging in a slight pause between the savoring of a new degree and appearing in print. The text is littered with typographical errors. Sentence structures often emerge in convoluted, and sometimes garbled, form. Verb tenses are occasionally mixed, and at least two sections of

footnotes (notes 119 to 124 on page 187 and 91 to 98 on page 367) are missing. Value judgments frequently appear extravagant or gratuitous. The doctoral candidate's understandable anxiety to establish thematic consistency sometimes leads to excessive repetition. The central argument—that Canada's policies in Africa from 1957 to 1971 were significantly affected by domestic politics—fits well enough with the current preoccupations of many political scientists in foreign policy analysis but may strike those who are detached from the discipline as a rather tiresome statement of the blindingly obvious. There is also a slight tendency to exaggerate the importance of Africa in Canadian affairs, both domestic and foreign—a tendency for which a more distant and leisurely perspective on the subject might well have been the cure. A thesis, in short, does not instantly make a polished book.

Readers who are prepared to overlook such blemishes will find in Schlegel's volume an extraordinarily useful examination of Canada's developing relationships with sub-Saharan Africa, a subject hitherto confined for the most part to a periodical literature of ephemeral quality. For reasons of economy, the book concentrates on Ghana, Nigeria (with special reference to the period of the civil war), "la francophonie," Tanzania, and Southern Africa, but this serves well enough to cover the most important ground, and from it the author has certainly been able to identify the principal features of Canadian policy. With the help of extensive interviewing he has also captured the flavor of much of the pertinent domestic Canadian politics. Given the "contemporary" character of the subject matter and the subtlety of the nuances involved, this is a considerable achievement, and perhaps especially so for a non-Canadian studying at Oxford.

American readers may be particularly interested in chapters 5, 6, and 7, which deal with "la francophonie" and, hence, with the complex and volatile dispute between Ottawa and Quebec (exacerbated by the interventions of France) over the role of the provinces in the conduct of foreign policy. The analysis, although somewhat qualified in the concluding chapter, could leave the uninitiated with the mistaken impression that the issue has been essentially resolved, when in fact it is still festering at the bureaucratic level and is almost certain to become visible again in the near future. A great many details, moreover, remain to be told. Nevertheless, Schlegel's treatment provides as clear a background on the problem as can be found anywhere in readily available English-language sources, and it may soon have a current, as well as historical, relevance for students of Canadian affairs.

It seems unfortunate that Schlegel did not delay his pursuit of a publisher long enough to permit a thorough revision of his text, and hence the creation

of a more mature and finely tuned result. Even as it stands, however, the book is a useful preliminary survey of a subject that thus far has received only sporadic attention in the literature of contemporary foreign policy analysis.

DENIS STAIRS

Dalhousie University

LATIN AMERICA

MARIA PARADOWSKA. *Polacy w Ameryce Potudiniowej* [The Poles in South America]. Wrocław: Ossolineum. 1977. Pp. 315. 55 Zł.

In this work Maria Paradowska provides many new insights into Polish immigration to Latin America, a subject much neglected by scholars outside Poland. Polish scholars have published much of the correspondence that went from Latin America to Poland in which immigrants described conditions in their new home. Although Paradowska indicates her intention to cover long periods of time and all aspects of life, her own ethnographic interests sometimes dominate the book. In preparing her book, Paradowska studied the writings of those who journeyed to Latin America, whether to emigrate or to report for newspapers in Poland. The most valuable part of the book is the extensive bibliography, which appears both in the back and scattered throughout the text. I am surprised, however, that she did not use the wealth of manuscript material available in the area since she acknowledges that rich materials about Poles in Latin America are available in foreign archives and libraries.

Paradowska's book starts long before the coming of Poles to settle in the territories of the Spanish empire; she begins with a short history of the area before the arrival of the Spaniards and Portuguese. Herein lies one of her weaknesses, for she covers too wide a range of topics and too many years. She analyzes Indian culture and lifestyle in a short section that is too general and that lacks innovative writing and research. Yet her account of the first news of Latin America in sixteenth-century Poland is interesting; she cites printed works from as early as 1506. Also of interest is the reference to Jan Dantyszek, who was sent by the Polish king to Spain. There he met with Hernando Cortez and the two men exchanged three "memorials" that are now lost.

The author also turns her attention to the question of whether the Poles discovered America. Many articles have already appeared on this subject and the quarrel about whether Jan Szkolny arrived before Columbus continues. Paradowska spends much time discussing reasons behind the emigration, immigrants' trials and difficulties in the new

lands, and individual and mass emigration up to the present. Also included are names of individuals who contributed to Latin American national development, biographical sketches, and histories of particular countries. There are some photographs and she does footnote some of her work.

This book suffers chiefly from the lack of manuscript material and adequate footnoting and from excessive generalization and superficiality in the coverage of some periods. Yet, for all its weaknesses, Paradowska's work does contribute knowledge in an area just being discovered, European immigration to Latin America.

BERNADINE PIETRASZEK
De Paul University

PIERO GLEJSES. *The Dominican Crisis: The 1965 Constitutionalist Revolt and American Intervention*. Translated by LAWRENCE LIPSON. Baltimore: Johns Hopkins University Press. 1978. Pp. xiii, 460. \$22.50.

Politics in the Dominican Republic, especially the civil war of 1965 and the accompanying U.S. intervention, have received so much attention from scholars and journalists that it is difficult to imagine anything fresh being said. Yet Piero Gleijeses has written an original and interesting book, bringing new information, insights, and interpretations to a well-known subject. He succeeds in his attempt to present the crisis of 1965 from Dominican points of view, as opposed to the "abundant literature . . . largely by American authors and primarily treating the American side of events" (pp. xi-xii).

This book is an expanded and revised version of the author's doctoral dissertation at the University of Geneva, translated from the original French. It is meticulously researched and impressively full of fascinating detail. In addition to a documentary base, Gleijeses interviewed a large number of participants in the events analyzed. The study is straightforwardly organized in chronological order. Chapter 1 provides sweeping historical background from the Spanish "discovery" through dictator Rafael Trujillo's assassination in 1961. Chapters 2 through 4 further set the stage, treating various aspects of the period from Trujillo's death to the Constitutionalist coup on April 24, 1965, against the Triumvirate government. Five subsequent chapters deal in virtual hourly detail with events of April 24-28, 1965. Chapter 10 sketches occurrences to June 1, 1966, and the presidential election of Joaquín Balaguer; chapter 11 summarizes the author's previously elaborated conclusions. Three appendixes are attached. One deals with the U.S. role in Trujillo's assassination, and another with the evolution of Dominican far-left parties until early 1962. The third appendix reproduces a contemporary Art Buch-

wald column satirizing U.S. interventionist policy in 1965. Ninety-seven pages of notes are included; much of the valuable commentary buried here would be better placed in the text. A twenty-four-page bibliographic essay is useful. The index is restricted to names of participants.

Glejeses analyzes the complicated mix of Dominican factions and U.S. involvement, sorting out personal interests and various policy motivations. His sympathies are clearly, sometimes emotionally and even passionately, with the Dominican left. He does not disguise his hostility to Dominicans identified with the status quo and to U.S. officials (John Bartlow Martin receives especially harsh criticism). But Glejeses does not fall into the trap of viewing the Dominican crisis in terms of heroic Constitutionalist pitted against villainous Loyalists, as did some authors of earlier works on the subject. He recognizes the simplicity of this approach and the remarkable complexity of Dominican politics, finding opportunism and idealism on both sides. While specialists will quarrel with some of Glejeses's provocative interpretations, he has made a major contribution to our understanding of the unique and enigmatic Dominican Republic.

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W. F. ELKINS. *Street Preachers, Faith Healers, and Herb Doctors in Jamaica, 1890-1925*. New York: Revisionist Press. 1977. Pp. 99. \$44.95.

W. F. Elkins has compiled in this volume what he calls "historical vignettes" on Jamaica. The book is one volume of what seems to be "studies in the Caribbean series." Although the book is weak in scholarship (most of his documentation being derived from newspaper articles in general and the *Jamaican Gleaner* in particular), the content is well written.

Elkins tried to present in simple style the historical development of the island from 1890 to 1925, in which he showed the tension existing between British colonial rule of law and order and the gradual development of political self-awareness in the Jamaican peasant community. This self-awareness emerged within what he called the "revival milieu," which came about as a synthesis of Afro-Jamaican possession cults; the Native Baptist tradition, which entered the island during slavery under the leadership of George Liele, an American slave; and Christian missionary religion. The combination of all three culminated in the Great Revival of 1860-61, in which the present religious milieu had its origin.

Elkins characterized revival movements as an "illusory inward escape from mundane concerns," but, at the same time, these movements retained a cer-

tain characteristic for political activities, which he called "a slumbering radicalism" that was very much "this-worldly." The revolutionary dimension of this movement precipitated the Sam Sharpe Rebellion of 1831, commonly known as the "Baptist War," and the Morant Bay Rebellion of 1865 under the two Native Baptist leaders, Paul Bogle and George William Gordon. Both of these rebellions brought about drastic changes in British colonial rule. The first contributed to the abolition of slavery, the second to the withdrawal of independence of Jamaica from the Plantocracy to Crown Colony.

Elkins further discussed various healers, both men and women, who emerged between 1890 and 1920, as curers of illness both by supernatural means and by the use of herbs and the emergence of the revival leader, Prophet Bedward, and his work at Mona River close to the University of the West Indies. He also traced the origin of the two most prominent sects in Jamaica, the Revival Zion and the Pocomania. The emergence of the Salvation Army through the works of W. Raglan Phillips and the rise of Ethiopianism, the forerunner of the present-day Ras Tafari movement, are also considered.

Despite weak scholarship in documentation, the book is delightful reading, neatly organized, and insightful to readers who have no knowledge of this rather exciting period in Jamaican history.

LEONARD E. BARRETT
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LAURENS BALLARD PERRY. *Juárez and Díaz: Machine Politics in Mexico. (The Origins of Modern Mexico.)* DeKalb: Northern Illinois University Press. 1978. Pp. xx, 467. \$25.00.

Laurens Ballard Perry's book is a long and detailed narrative of the political and military history of the Restored Republic (1867-76). The era was one of intense factional conflict and electoral corruption under Presidents Benito Juárez and Sebastián Lerdo de Tejada, which culminated in a three-way struggle for the presidency in 1876 among the incumbent Lerdo, the president of the Supreme Court and vice-president, José María Iglesias, and the successful rebel, Porfirio Díaz. The book is the product of intensive research in the Díaz archives (which the author himself organized for public use) as well as in the papers of Juárez, Justo Benítez, and Fernando Iglesias Calderón and in some major newspapers. Perry's volume supplements the works of Frank Knapp and Daniel Cosío Villegas and adds much new information on state politics and on electoral practices; moreover, it may become the definitive account of the complicated events of 1876.

The triumph of Díaz came not by default but as a result of a careful military plan and the use of guerrilla tactics learned in the struggle against Maximilian.

The central argument of the book is that the real conditions of Mexico in 1867 made it impossible for Juárez and Lerdo to follow the ideals of "liberal republicanism" as enunciated in the Constitution of 1857 and the Laws of Reform. Federalism, a limited presidency, and separation of powers gave way to "executive centralism"; democratic electoral procedures to fraudulence and machine politics; social democracy to rural abuse. Perry argues further that the gap between liberal ideals and actual political practice gave stimulus to a rebellious opposition, culminating in the Díaz victory. Juárez and Lerdo, though publicly clinging to liberal theories, contradicted them in practice and thus were little different from President Díaz after 1876. Perry adheres to the theme of continuity from Restored Republic to Porfiriato, suggested by Emilio Rabasa (1912) and Knapp (1951) but denied by Cosío Villegas (1955). In fact, much of the author's discussion seems a refutation of Cosío's contention that the Restored Republic represented the high point of liberal constitutional government in Mexico.

The strengths of the book—its impressive research and its wealth of detail—are nonetheless undercut by conceptual weakness and confusion. The author's argument rests on demonstrating the inadequacies of the "liberal model" as a guide for post-1867 governments, and yet he never really plumbs the complexities of political liberalism. For him, it seems to mean solely the limitations on central government authority outlined in the Constitution of 1857. What Perry ignores is that political liberalism also entailed the secularization of society and the destruction of corporate inequality. This was the program of the Reforma, one recognized by the reformers themselves as anticonstitutional because it necessitated the strengthening of the state. The "executive centralism" of Juárez and Lerdo after 1867 had its direct antecedent in the ideals and practice of the Reforma. Political liberalism in Mexico, as in France and Spain, has always contained two contradictory "models," constitutionalism and the secular state. Juárez and Lerdo (and Díaz) could thus be centralists and also good liberals. The author seems insensitive to the subtle interaction between theory and practice in the political system of the Restored Republic (and in the Porfiriato) and particularly to the effects of liberalism as an all-embracing myth following the heroic liberal age. Moreover, Perry's account shows traces of the traditional Anglo-American judgmental attitude toward Latin politics, making it reminiscent of the old studies of the Spanish colonial system that emphasized the breach between the written law and its

observance. Despite these flaws, the book will stand as an important reference for the politics of the years 1867–76.

CHARLES A. HALE
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GUILLERMO HERNÁNDEZ DE ALBA and JUAN CARRASQUILLA BOTERO. *Historia de la Biblioteca Nacional de Colombia*. (Publicaciones del Instituto Caro y Cuervo, number 38.) Bogotá: The Institute. 1977. Pp. xviii, 447.

The Biblioteca Nacional in Bogotá is the oldest national library in the Americas, indeed, is older than any but about a half dozen national libraries in Europe. With the expulsion of the Jesuits in 1767, their property was turned over to the crown. The society's library in Santafé de Bogotá contained 4,182 volumes. Largely at the instance of the creole Francisco Antonio Moreno y Escandín, the Real Biblioteca of Bogotá opened its doors in 1777. The first of sixty-three librarians through the bicentennial was Anselmo Álvarez. Among them have been such brilliant names as Miguel Antonio Caro, Daniel Samper Ortega, and Tomás Rueda Vargas.

Here is the first chronologically complete history of the first two centuries of the library. Eduardo Posada, the noted Colombian bibliographer and historian, published *Narraciones: Capítulos para una historia de Bogotá* in 1906, with one chapter on the library, the best available study to date. The present work uses many documents and much iconographic material that could not find a home in Posada's relatively short work. The authors have used original documents (manuscript and printed), new printed editions of sources, and many portraits, photographs, and facsimiles (74 plates in all). This book is mainly a framework, however, and the critical analysis of the past history of the Biblioteca Nacional and its prognosis for the immediate future is a chore that remains to be done.

The story of the Latin American national libraries, even the fairly large ones in Mexico and Rio de Janeiro, is, in general, pathetic. They have received nothing like the support given to European national libraries, even in the smaller jurisdictions. They have been political booty for men of letters and other persons on the right side (as have many state libraries, even today, in the U.S.). The present work reflects this situation, but it is an eligible point of departure for future studies, particularly with the indication of source material. There are only bibliographical footnotes, not a bibliography.

There is a section on special collections. Like most of the recent U.S. "research" libraries, the collections are spotty, with isolated cimelia. Most valuable are the collections on Colombian literature and history, of which a catalogue would be useful. A few

of the other special collections also deserve to be made better known through catalogues.

The gradually increasing interest in the library history of Latin America is most encouraging. Perhaps more than most institutions, libraries can profit from the examination of the past, for the physical evidence of achievements and mistakes is on all sides and must be handled on a continuing basis. Here is a cornerstone for Colombian library history.

LAWRENCE S. THOMPSON
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JONATHAN C. BROWN. *A Socioeconomic History of Argentina, 1776–1860*. (Cambridge Latin American Studies, number 35.) New York: Cambridge University Press. 1979. Pp. xiv, 302. \$24.95.

A Socioeconomic History of Argentina, 1776–1860 is actually a study of agriculture and commerce within the province of Buenos Aires. The author ably relates the provincial economy to the riverine, interior, and other littoral provinces and to the world market. Jonathan C. Brown enters the dependence theory controversy by stating that it provides an unsatisfactory explanation of Argentine development. He concludes that the more realistic explanation of Argentine economic growth is found in staple theory, which proposes that the production of raw materials and food crops rather than creating a neocolonial situation provides the basis of development.

Brown's research is impressive both in utilization of Argentine archival sources and in examination of printed materials. He imaginatively utilizes information relating to commerce and agriculture that has been ignored and concisely summarizes relevant literature, thereby presenting for the first time a whole picture of the nineteenth-century agricultural and commercial operations within the province of Buenos Aires.

This, however, is a disappointing book because the title leads one to expect a great deal more. In no sense is this work a socioeconomic history of Argentina. Brown provides no clear picture of Argentine mining and industry. He fails to describe the financial infrastructure. He presents rather limited information on immigration and the family and nothing on religion, education, or social classes. Furthermore, one does not find information on agriculture within the riverine and interior provinces, although Brown's discussion of the links to the province of Buenos Aires and the benefits that the other provinces received from their association supports his analyses of staple economic growth. The author favors the thesis that the independence movement did not immediately bring about the decline of the riv-

erine provinces. The closer the provinces to Buenos Aires, the greater the economic growth.

Brown should have titled his work "The Developments of Agriculture and Commerce of the Province of Buenos Aires, 1776-1860." He describes the growth of Buenos Aires trade from contraband to legitimacy, changes in trade patterns with the coming of independence and increased technology, the river and cart routes, and the expanding frontier of the province of Buenos Aires. Of particular interest are accounts of the provincial links to Potosí and Asunción and descriptions of the European industrial market for Argentine wool, hides, tallow, and meat. Brown also provides the first detailed information on *estancia* management through the examination of the Anchorena family operations. He challenges traditional theories of land patterns by proving decreasing size of holdings in the nineteenth century and arguing for the significance of the small holder and the ease of land purchase. The nineteenth-century watercolor and oil painting illustrations are excellent as are the thirty-four tables and six maps and graphs.

Jonathan Brown has made a contribution to the economic history of Latin America, and his work should be consulted by those working on nineteenth-century trade and agriculture. However, a socioeconomic history of Argentina remains to be written.

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ROBERT J. ALEXANDER *Juan Domingo Perón: A History*. Boulder, Colo.: Westview Press. 1979. Pp. xiii, 177. \$15.00.

The death of Argentine President Juan D. Perón in July 1974 put an end to the career of that country's most controversial personality of the present century. Robert J. Alexander, who published one of the earliest studies on Perón's first period of rule (*The Perón Era* [1951]) has now produced what is apparently the first book in English to cover Perón's entire political career.

Alexander's new work is not a biography in the usual sense. The personal side of Perón's career is treated in a chapter entitled "The Young Perón" and briefly in other chapters, especially one devoted to his second wife, Evita, but there is no attempt to delve deeply into the psychological factors that may have explained Perón's actions. Rather, what Alex-

ander has done is to present in this slender volume a narrative and interpretive study of Argentine history from 1943 to 1974. The bulk of the volume deals with the years leading up to Perón's ouster from power in 1955, but the last forty-three pages treat the years in exile and the return to power in the 1970s as well as offer an overall assessment of Perón's role.

For those unfamiliar with recent Argentine history, this book can be recommended (save for the reservation noted below) as providing a useful summary and interpretation of the Perón-dominated years. Alexander, who visited Argentina thirteen times between 1946 and 1974 and conducted perhaps a thousand interviews, offers explanations for many crucial developments of those years, and he does not hesitate to take firm positions on such perennially intriguing questions as to whether it was Perón or Evita who was the more important in their political partnership. Alexander's judgments about Perón reflect his own social democratic preferences, but this book is far more sympathetic to an understanding of Perón's economic and social policy objectives than his earlier volume. His conclusion that Perón has a tragic figure rather than a great one and that he directed his great political talents more often to destructive than to constructive ends will seem persuasive to many who try to understand the tragedy of recent Argentine history.

In the preface to his work, Alexander describes it as "an interpretive essay rather than a scholarly treatise" and therefore "unencumbered by the paraphernalia of footnotes and other scholarly impedimenta" (p. xi). The absence of scholarly apparatus, however, is no excuse for the numerous errors of fact that appear in the text. The author is quite cavalier in his use of dates (see pp. 1, 12, 18, 25, 56, 104, 107, 117); he is also mistaken in a number of matters of varying importance from the role of General Aramburu during and after the 1955 revolt to the details of the 1962 election that led to President Frondizi's ouster. In what is perhaps the most notable confusion, Alexander describes as occurring on a single day, June 15, 1955, events that happened two years apart on April 15, 1953, and June 16, 1955 (p. 104). These are the kind of lapses one might expect to find in the first draft of a book of recollections, not in an edited volume presented to the public under the title of a history.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

PAUL GORDON LAUREN, editor. *Diplomacy: New Approaches in History, Theory, and Policy*. New York: Free Press. 1979. Pp. xvi, 286. \$25.00.

PAUL GORDON LAUREN, *Diplomacy: History, Theory, and Policy*. GORDON A. CRAIG, *On the Nature of Diplomatic History: The Relevance of Some Old Books*. ALEXANDER L. GEORGE, *Case Studies and Theory Development: The Method of Structured, Focused Comparison*. MELVIN SMALL, *The Quantification of Diplomatic History*. OLE R. HOLSTI, *Theories of Crisis Decision Making*. SAMUEL R. WILLIAMSON, JR., *Theories of Organizational Process and Foreign Policy Outcomes*. RICHARD SMOKE, *Theories of Escalation*. PAUL GORDON LAUREN, *Theories of Bargaining with Threats of Force: Deterrence and Coercive Diplomacy*. ROBERT JERVIS, *Systems Theories and Diplomatic History*. ROGER V. DINGMAN, *Theories of, and Approaches to, Alliance Politics*. SAMUEL F. WELLS, *History and Policy*.

PAWEŁ CZARTORYSKI *et al.*, editors. *Science and History: Studies in Honor of Edward Rosen*. (Studia Copernicana, number 16.) Wrocław: Ossolineum; distributed by Neale Watson Academic Publications, New York. 1978. Pp. 553. \$35.00.

PAWEŁ CZARTORYSKI, Foreword. PAUL OSKAR KRISTELLER, *The First Printed Edition of Plato's Works and the Date of Its Publication* (1484). MARSHALL CLAGETT, *Francesco Maurolico's Use of Medieval Archimedean Texts: The *De sphaera et cylindro**. CHARLES B. SCHMITT, *Filippo Fantoni, Galileo Galilei's Predecessor as Mathematics Lecturer at Pisa*. VASCO RONCHI, *Two Thousand Years of the Struggle between Reason and the Senses*. NICHOLAS PASTORE, *"In His Eye, or Rather In His Mind."* ERNEST NAGEL, *The Supremacy of Method*. A. I. SABRA, *An Eleventh-Century Refutation of Ptolemy's Planetary Theory*. PEARL KIBRE, *"Astronomia" or "Astrologia" Ypocratis*. OLAF PEDERSEN, *The Decline and Fall of the Theorica Planetarum: An Essay in*

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

I read Walter J. Brunhumer's review of my *Leibniz and Confucianism: The Search for Accord* (*AHR*, 83 [1978]: 1040–41) with dismay. I accept his criticisms regarding the lack of development of Leibniz's personality and the weaknesses in my argument concerning the reasons why the search for accord failed. But I find it harder to accept his omission of reference to chapters 4 and 5, which, as other reviewers have correctly noted, constitute the core of the book. These chapters trace the transmission of Chinese concepts from the Chinese texts through the missionary translators to Leibniz, and this material is the basis for saying that my book is one of the "first works to begin to document the relationship between the two worlds" of China and Europe that began in the sixteenth and seventeenth centuries (*Choice*, 15 [May 1978]: 318–19).

I do not fault Professor Brunhumer for his treatment of European intellectual history. I fault him for his avoidance of Chinese intellectual history. From his dubious postulation of Chinese parallels to the European faith-reason tension, it appears that he has little understanding of China. In fairness, the problem is not the reviewer's alone. Many historians continue to resist merging areas of expertise, a resistance that is reflected in the *AHR*'s review editors giving the book to a European historian, rather than finding one with a dual (European-Chinese) expertise, and publishing the review under the "Europe" instead of the "General" category. (In doing so, the *AHR* ignored the Library of Congress classifications on the second page, of which three out of four refer to China.) The problem is that, unless we begin to merge areas of expertise, we are never going to appreciate the rich confluence of histories, such as occurred between Europe and China, which

is increasingly crucial to an understanding of the modern world. Some of the intellectual fences that historians have built between Europe and China are not part of history.

Writing a disagreeable letter over a review of one's own book is surely not the best way to open minds to a new approach. Furthermore, by writing it I realize that I wave the banner of self-interest. But an important issue is involved. And, since I did try first to write a book that would prod historians in that direction and since the primary objective of the book has been omitted from the review, perhaps this letter may be received more in the spirit of scholarly emendation than of authorly revenge.

DAVID E. MUNGELLO
Leibniz-Archiv
Hannover, West Germany

PROFESSOR BRUNHUMER REPLIES:

In his letter as in his dissertation, David E. Mungello displays a distressing tendency to jump to conclusions on the basis of misunderstood evidence. Apparently persuaded that it is his mission to expose the self-evident evils of "sinological torque" (Western misconceptions of Confucianism), and overlooking the fact that its contrary (asinological torque?) is equally unlikely to advance the cause of an East-West entente, he gratuitously and presumptuously makes his own failure to penetrate to an even rudimentarily cogent interpretation of Leibniz's ethical thought into "The Failure of Leibniz" Philosophy." That the alleged "unresolved tension" therein, which better-equipped scholars have not found at all irresolvable, was the underlying reason for the collapse of the "search for accord" is the very thesis of the dissertation, which obviously concerns not *merely* Chinese thought or *merely* Leibniz and Western thought but precisely the connection between the two. Inevitably, then, this thesis attracted prime attention in all of the (remarkably few) reviews that have appeared, and it has escaped severe criticism only in *Choice*, whose reviewer seems unaware of the work, among others, of Needham, Rowbotham, Merkel, Dunne, Bernard, Zemlinier,

Franke, Demiéville, Grimm, and, especially, Donald Lach.

The burgeoning literature on the relationship between Maoist and "classical" Confucianist China makes manifest the superficiality of regarding Chinese philosophy as a tensionless and static repository of wisdom unaffected by external circumstance. "Oh the patience of the ageless Chinese with their external values!" Jonathan Spence has sighed. "One grows a bit weary." But Dr. Mungello, though he wishes to assist our understanding of the modern world, is oblivious to annoying historical complexities while smugly pontificating about pioneering dual "areas of expertise." Comparative studies in East-West philosophy are actually less fashionable today than they were thirty years ago—by which time, incidentally, I had already long since completed an extended and intensive program in the navy's Oriental Language Institute, which first sparked my interest. His dissertation advisers should have warned Mungello that a leap from the seventeenth to the twentieth centuries that blithely ignores all that intervened, whether East or West, predictably produces problems owing to certain limitations on how long underfueled generalizations can remain airborne. It is interesting that Thomas Metzger—whose *Escape from Predicament: Neo-Confucianism and China's Evolving Political Culture* (1977), however controversial it may be, exemplifies the ferment that is induced by writers of subtle intellect who really *can* handle the intricacies of a multiple approach—has dwelled upon the "rationalistic bias" of Neo-Confucianism, which he has found fairly bristling with ambiguities. The inclination of Confucianists to consider polarities as complementary rather than antithetical does not change the fact that "tensions existed between the poles in question [and] that some men gravitated to . . . one pole rather the other," wrote Benjamin Schwartz in 1959; and in 1974 Julia Ching discerned in the diverse approaches of Chu Hsi and Lu Hsiang-shan a "parallel" to the Dominican-Franciscan dispute over the nature of Christian spirituality. (Need one add that a parallel is not an identity?)

More space is given in my review to the portions of the dissertation that I am charged with ignoring than is allotted them by the reviewer whose phrase ("core of the book") the author borrows! Readers of the *Review* were clearly informed as to what is central in the book—something to which attention can be drawn by less mechanical means than reciting chapter numbers. Dr. Mungello's statement on this point is therefore, to put it mildly, deliberately misleading. In fact, virtually every sentence of his communication, which evidently was hastily composed, contains some species of misrepresentation. The letter is not merely disagreeable; it lacks integrity, for which reason its motivation is rendered all the more

palpable by its author's rather too overt protestations.

WALTER J. BRUNHUMER
Western Michigan University

TO THE EDITOR:

I have met two or three intellectual giants in my thirty-odd years of academic work; one of them is Karl August Wittfogel. Perhaps David Felix, author of the review of G. L. Ulmen's biography of Wittfogel, *The Science of Society* (AHR, 84 [1979]: 1026-27), has never met any. Though Felix declares that "Wittfogel deserves . . . a sympathetic but critical study that would evaluate his achievements credibly," he seems chiefly concerned to attack the subject of the study. Credibility for his own review is difficult to claim, since he first declares that Wittfogel merely "enlarged upon a casual and ignorant aside of Marx about the 'Asiatic mode of production'" (thereby raising the question of whether Felix knows how much Marx wrote about Asia and Russia, using the concept in question) and then declares Wittfogel's views to be "overwhelmed . . . by the grand architectonics of Marx"—which presumably pertain to Europe alone, as Marx himself insisted his famed sequence of slavery, feudalism, and capitalism did. *Oriental Despotism*, writes Felix, was "almost immediately abandoned by serious scholarship"; a few lines later, Wittfogel's exploration "has encouraged fruitful scholarly inquiry." Wittfogel is said to have "provided no data on the location, size, management, and relation to government and society of the hydraulic works"; I refer Felix, as a start, to *Zweiter Abschnitt, Wirtschaft und Gesellschaft Chinas* (1931), followed by much more.

Wittfogel—whatever the extent of his debt to the "ignorant" Marx and also, as Felix says, to Weber—has raised more forcefully than anyone else, living or dead, the fundamental issue of whether the world outside of Europe and Japan has been dominated by social and political formations sharply different from those of the West and thereby has suggested cogent reasons why communism has succeeded and Western democracy has apparently failed in many countries of the area. (If such a view is "Eurocentrism," Felix had better explain why.) My sentence is an inadequate characterization of Wittfogel's work, but it is a much less inadequate one than is offered by Felix's unhappy review.

DONALD W. TREADGOLD
University of Washington

TO THE EDITOR:

Joe Gray Taylor's review of Peyton McCrary's *Abraham Lincoln and Reconstruction: The Louisiana Experiment* (AHR, 84 [1979]: 1161-62) states of McCrary's conclusion that Lincoln was aligning himself shortly before his death with congressional Radical Re-

publicans on Reconstruction policy, "So far as this reviewer is aware, this contradicts the conclusion of all other reputable scholars who have studied Lincoln's attitudes."

In my opinion, a close examination of Professor McCrary's documentation illuminates the existence of a pride of "reputable scholars" who agree with McCrary. I do, at least, and I do not feel lonely.

HAROLD M. HYMAN
Rice University

ERRATUM:

In Lewis A. Tambs's review of *The Bandit King: Lampião of Brazil* (*AHR*, 84 [1979]: 1512), the author of the book, Billy Jaynes Chandler, is incorrectly given an affiliation with Texas A&M University. In fact, Professor Chandler is a member of the department of history at Texas A&I University at Kingsville. The editors regret the error, which is the fault of the *AHR* and not Professor Tambs.

THE EDITORS

Recent Deaths

SOLOMON M. LUTNICK, associate professor of history at Queens College, died on September 13, 1979, after a long illness that he approached with the bravery and good humor so characteristic of him. Professor Lutnick was born in New York City on September 6, 1928, and spent all of his academic life there. He received his A.B., M.A., and Ph.D. degrees from Columbia University, being awarded the latter in 1960. At Columbia he studied with Professor Richard B. Morris and specialized in the American Revolution, a field in which he displayed unending interest. After teaching briefly at Hunter College, Professor Lutnick came to Queens College in 1956 and remained there until his death.

Dr. Lutnick is best remembered for his book, *The American Revolution and the British Press, 1775-1783*, published in 1967 by the University of Missouri Press. The book traced the split in English thought during the troubles with the American colonies, pointing up the enormous sympathy the British people held for their countrymen overseas. More recently, Professor Lutnick was interested in Edward Gibbon and before his death was collecting material for a biography of that great historian.

As a teacher, Professor Lutnick can only be described as superb. Approaching American history with an enthusiasm and depth seldom matched, he attracted large numbers of undergraduates to his classes. During his career he taught close to six thousand students, leaving his imprint on almost every one of them.

Dr. Lutnick will be remembered as a devoted scholar, a fine teacher, and a warm human being.

STANLEY P. HIRSHSON
*Queens College,
City University of New York*

JOSEPH HENDERSHOT PARK, professor emeritus of history and dean emeritus of the Graduate School of Arts and Science at New York University, died on October 6, 1979 at the age of 89 in the Morristown Memorial Hospital in New Jersey. Born in Port Murray, N.J., he attended Williams College for two

years and went to Columbia University for his A.B. in 1912, A.M. in 1913, and Ph.D. in 1920. He joined the faculty of New York University in 1915, saw overseas service in World War I, and returned to rise to full professor by 1928. He became chairman of the department of history at University College and executive secretary of the Graduate School of Arts and Science in 1940. In 1943 he was named dean and head of the graduate department of history.

Park's major publications were *The English Reform Bill of 1867* (1920) and *British Prime Ministers of the Nineteenth Century: Policies and Speeches* (1950). He contributed a number of articles on English history and on antiques and ceramics. Internationally known as an authority on Wedgwood chinaware, he also bred new types of flowers, notably peonies.

Few persons, if any, at New York University enjoyed the affection and respect that Park did. Students, faculty, clerical personnel, and administrators turned to him for guidance and sympathy. His wisdom, integrity, compassion, and cheerful outlook seemed as unfailing as his energy. Old-fashioned in dress and lifestyle, he was open to new ideas and methods and was thus an ideal official to preside over the great expansion of the department of history and the graduate school after 1945. Without being a crusader, he broke barriers of race, religion, and sex discrimination. The growth of the university's prestige owed much to his stewardship.

On a typical day Park commuted for an hour and a half from New Jersey by rail, ferry, and subway to Washington Square to attend to his graduate duties. At noon he would take the long subway trip to University Heights, where he spent perhaps his happiest hours teaching large classes in English history. Then he would return to the Square for administrative labors that grew heavier each year. He presided over every oral examination in his department, and he served on almost every committee in the university. No bureaucrat, he carried important information scribbled on slips of paper in his pockets. Yet he seemed to know everything and to forget nothing. Superlatively effective though he was as an

administrator, Park loved teaching best, and his students responded enthusiastically to his learning, wit, and incomparable personality.

After retiring in 1955 Joseph Park continued to be active as a gardener, farmer, and collector. Until the last he played the organ at a rural Methodist church near his native Port Murray. Nearly a quarter-century after his retirement his contributions to New York University are widely recalled with admiration and gratitude.

JOHN E. FAGG
New York University

FRANCE VINTON SCHOLES died in Albuquerque, New Mexico, on February 11, 1979, shortly after celebrating his eighty-second birthday on January 26. Born in Bradford, Illinois, he received the A.B. from Harvard University in 1919, the A.M. in 1922, and the Ph.D. in 1943. He taught history at Massachusetts Institute of Technology in 1919–20, at the University of New Mexico in 1925–26, and again in 1928–31, and at Colorado College in 1926–27. He had gone to the Southwest as a health seeker, and for him, like many who found a cure for tuberculosis there, New Mexico became his beloved adopted homeland.

His long career as one of the most distinguished research scholars of colonial New Spain and the Hispanic Southwest began in 1927–28 when Harvard awarded him the Woodbury Lowery Fellowship for travel and archival research in Mexico and Spain. In 1929–30 he was director for the Library of Congress of a project for photo-reproduction of documentary resources in Mexican archives. His extraordinary flair for fruitful archival investigation soon become evident and led to many spectacular finds then and later. By the time he joined the staff of the Division of Historical Research, Carnegie Institution of Washington, as head of the Post-Colombian History Section in 1931 he had discovered so much important new documentation for the history of New Mexico and the Spanish Southwest that it has not been fully exploited to this day. His own pioneering publications on seventeenth-century New Mexico remain valid and indispensable for the study of the period, and he continued to add to them throughout his life.

Carnegie, however, gave his research a new and equally profitable direction. Under Alfred Vincent Kidder, also a New Mexico expert, the Division of Historical Research undertook a major interdisciplinary program of study of Maya culture in all its aspects. During his fifteen years with the Carnegie Institution, Scholes's research again resulted in enormous quantities of new material and major finds to illuminate and revise traditional notions of the Maya past. His work and publications in this

field are invaluable, and his famous colleague, Maya archeologist J. Eric S. Thompson, paid fitting tribute when he numbered Scholes "among the giants of this century." The scope and depth of his interests were not limited to the Spanish Southwest and the Maya area. He also collected masses of material concerning colonial New Spain proper, some of which saw publication. By the time he died his collection of source material for the life and times of Hernán Cortés may well have been the most complete extant. Unfortunately, he was unable to finish the life of the conqueror with which he had hoped to crown his career as a historian.

In 1946 he returned to the University of New Mexico, where he served as dean of the graduate school, 1946–49, and first academic vice president, 1948–56. He was a fellow of the American Academy of Arts and Sciences and a corresponding member of the Academy of American Franciscan History, which honored him with the Serra Award of the Americas in 1956. After his retirement he was a Bollingen research fellow 1962–65, and visiting professor at Tulane University, 1968–69.

To France Scholes the life of the scholar was a joyous adventure. He was a dedicated perfectionist who loved and respected his profession and was rigid in his belief that complete honesty and objectivity are essential. Whatever the drudgery—and archival research is often a dreary, dusty task—he always found it fun and exciting. But Scholes was more than an outstanding investigator and writer, he was an inspiring teacher. One of his greatest achievements was to pass his integrity and enthusiasm on to his colleagues and students. In their gratitude for his guidance and assistance the same themes emerge over and over again: "A great historian, but also a friend of inexhaustible kindness and generosity, who made the practice of history an uninterrupted joy for himself and others." "I would hope that our contemporaries and successors will approach scholarship and teaching with the same breadth of vision, tolerance, and urbaneness, not to ignore simple generosity." "He represented pure and dedicated scholarship at its highest level." "He was truly an inspiration to me (as he was to so many others) and I resolved then and I resolve now to try harder to be the kind of professor and scholar he would have me be." "Papa Scholes" will not soon be forgotten by those who enjoyed the privilege of his company.

ELEANOR B. ADAMS
*University of New Mexico,
Albuquerque*

DAVID M. VIGNESS, professor of history at Texas Tech University, died July 16, 1979. Born in La Feria, Texas, on October 12, 1922, he served as a lieutenant

ant in the United States Navy in the Pacific Theater during World War II. He received his bachelor's (1943), master's (1948), and doctor's (1951) degrees from the University of Texas at Austin. From 1951 to 1955 he served as head of social sciences at Schreiner Institute in Texas. He then joined the faculty at Texas Tech University, where he chaired the department of history from 1961 to 1978, an important period of growth and diversification. He served as a Fulbright Lecturer at the University of Chile and the Catholic University of Santiago in 1957–1958. During the spring of 1979 he taught as a visiting professor at the University of New Mexico.

David's scholarly interests spanned the history of nineteenth-century Mexico, the Southwestern borderlands, and Texas. His published works included several articles and two books, *Documents of Texas History* (1963), edited with Ernest Wallace, and *The Revolutionary Decades* (1965) in the Saga of Texas series. Honors for his scholarship included election as a fellow of the Texas State Historical Association and receipt of the H. Bailey Carroll award for the best article in the *Southwestern Historical Quarterly*, 1971–72.

His participation in several professional organizations included the presidency of the Southwestern Council of Latin American Studies, membership on the executive council of the Southwestern Social Science Association, and the vice-chairmanship of the Texas Committee for the Humanities. Furthermore, he served on the editorial boards of several scholarly journals.

David also participated in the work of many university committees, in a number of community organizations, and as an active member of the Presbyterian Church. Perhaps he will be remembered most of all, however, as a kind, even-tempered man who as a chairman and as a professor always found time to counsel with faculty and students.

ALWYN BARR
Texas Tech University

SERGIUS O. YAKOBSON, scholar, author, librarian, specialist in Russian history and Slavic languages, ranking Slavist in the Library of Congress, and foreign policy adviser to the Congress for over 30 years, died on November 13, 1979, at the George Washington University Hospital.

Born in Moscow in 1901, Yakobson received his early academic training at the Lazarev Institute for Oriental Languages in Moscow. Following graduation in 1918, Yakobson fled with his family to Germany in the wake of the Bolshevik Revolution. There, he earned a Dr. Phil. degree at the University of Berlin in 1926 majoring in history and with a minor in Slavic languages, literature, and philosophy. For the next seven years Yakobson was a re-

search associate in the Prussian Privy State Archives, but as a Jew he was forced to leave Germany in 1933 and take up residence in England. With the assistance of Arnold Toynbee, he was appointed Honorary Lecturer in Russian History at the University of London and Chief Librarian at the University's School of Slavonic and East European Studies. During his London years covering the period 1933–1940, Yakobson contributed to publications of the Royal Institute of International Affairs and to British scholarly journals; he lectured at Oxford, Cambridge, Nottingham, and Cardiff; and along with his many scholarly activities he enjoyed a professional association with such notable British scholars as Sir Bernard Pares and Toynbee.

In 1940, Yakobson came to the United States and a year later was appointed a Rockefeller Fellow in Russian History at the Library of Congress. Thus began an association with the Library that was to last for 38 years. During most of that time Yakobson served simultaneously in the Library's Congressional Research Service as Senior Specialist in Russian Affairs and in the Slavic and Central European Division as its Chief. Upon retirement in 1971, he was appointed the Library's Honorary Consultant in Slavic Studies, a post he continued to hold until his death.

As a scholar in Slavic studies recognized internationally, Yakobson combined a professional career of service in the United States government and to the scholarly community. During World War II he was second in command of a confidential White House project on migration and settlement. As Chief of the Slavic Division, he was the primary force in developing and expanding the Library's Slavic collection. As Senior Specialist in Russian Affairs, he contributed enormously to the proceedings of the Congress by his written work and oral consultations with members and staff. And in addition to his official duties he served the scholarly community directly: by lecturing at some of the nation's leading universities; by presenting research papers at national and international meetings on subjects of scholarly concern; by actively serving in such eminent organizations as the Joint Committee on Slavic Studies of the American Council of Learned Societies, the American Historical Association, and the American Association for the Advancement of Slavic Studies—(In 1972, the AAASS presented Yakobson its National Award in recognition for his contribution to Slavic studies); and finally publishing the results of his extensive research. Listed among Yakobson's many publications both by the government and in the private press is his chapter on "Russia and Africa" included in *Russian Foreign Policy: Essays in Historical Perspective* published by the Yale University Press in 1962. This essay, leavened by years of research and reflections on Af-

rica, has now been recognized as a pioneering effort in tracing Russia's historical interest in Africa.

In his long professional career, Yakobson was thus a working scholar motivated by a high sense of service to his government and to the scholarly community, applying his skills with a dignity, dedication, and integrity that won for him the respect and gratitude of those he served. In a life that had been shaped by training and experience in three great

cultural traditions of the world—the Slavic, the Germanic, and the Anglo-Saxon—he was unique as a “scholar-in-government” for he could and did draw upon the best of all three traditions and thus made a distinctive contribution to the vital center of American life.

JOSEPH G. WHELAN
*Congressional Research Service,
Library of Congress*

American Historical Association

Founded in 1884. Chartered by Congress in 1889
Office: 400 A Street, S.E., Washington, D.C. 20003

President: David H. Pinkney, *University of Washington*
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MEMBERSHIP: Persons interested in historical studies, whether professionally or otherwise, are invited to membership. The present membership is about 17,500. Members elect the officers by ballot.

MEETINGS: The Association's annual meeting takes place on December 28-30. The meeting in 1980 will be held in Washington, D.C. Many professional historical groups meet within or jointly with the Association at this time. The Pacific Coast Branch holds separate meetings on the Pacific Coast and publishes the *Pacific Historical Review*.

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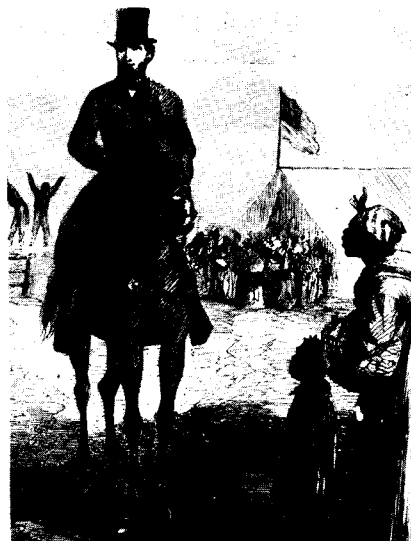
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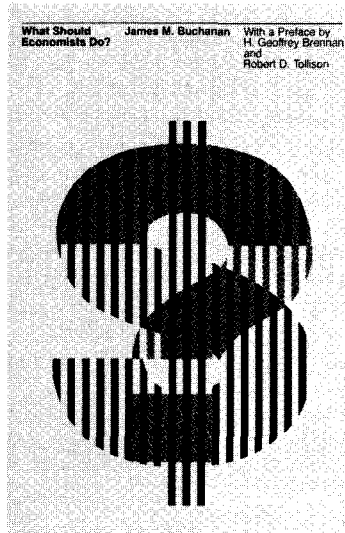
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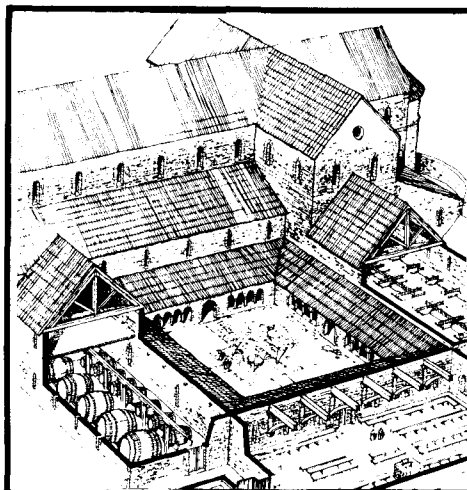
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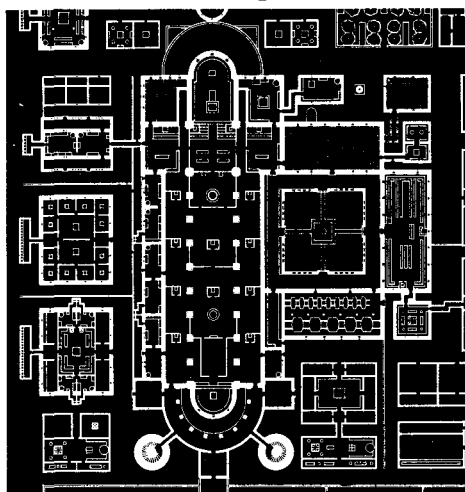
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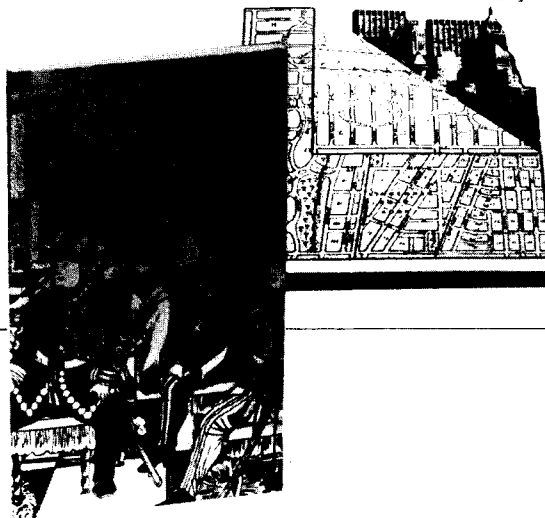


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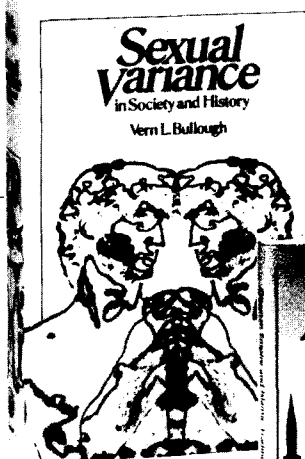
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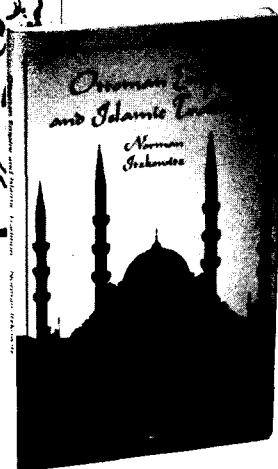
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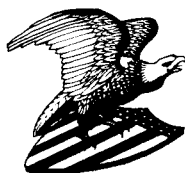
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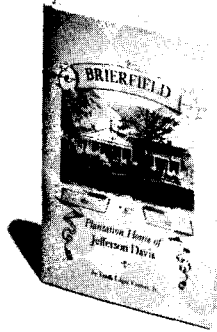
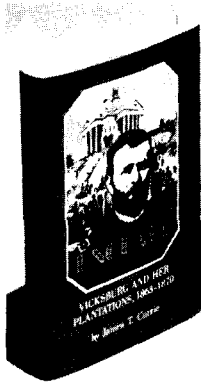
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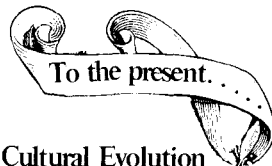
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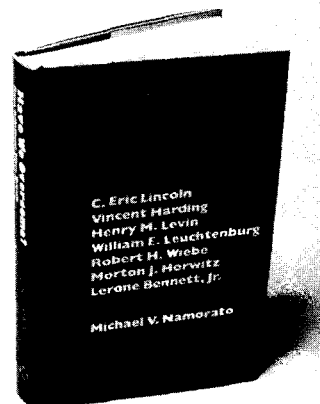
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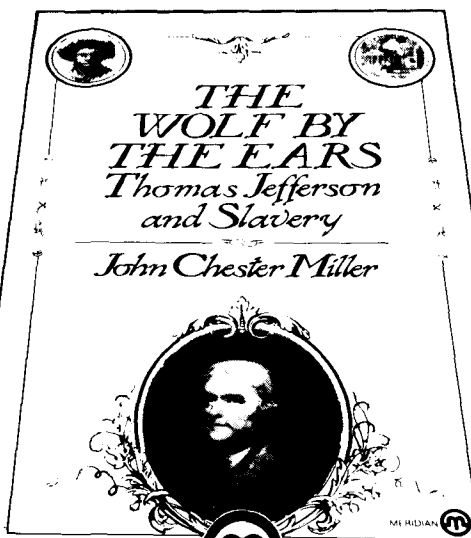
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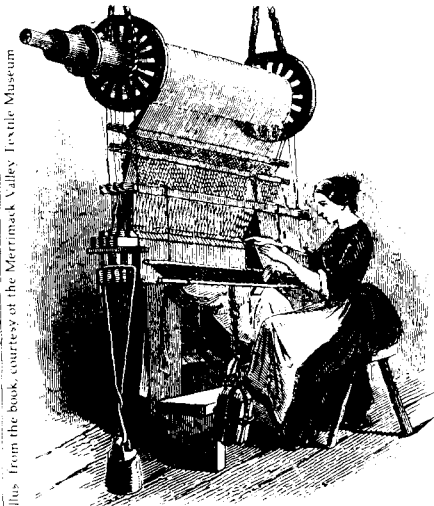


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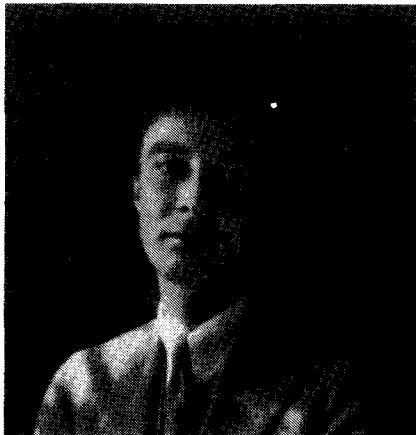
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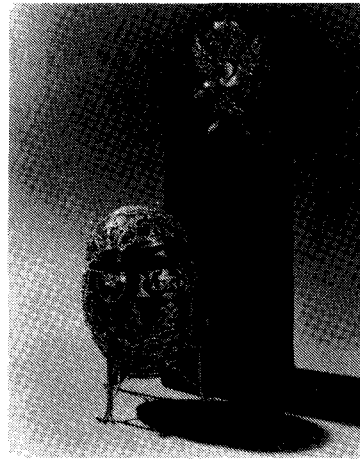
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